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LIEBIG,
HIS CHEMISTRY AND REVIEWERS.

BY

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No. X.

The Mixed Diet in daily use by Mankind, consists of the same organic proximate principles which constitute the food both of carnivorous and herbivorous animals; of principles, that is to say, belonging to the series of elements of respiration, and to that of the plastic elements of nutrition, as they have been designated by Professor Liebig.

Where the dictates of nature are not violated, the quantity of food, its qualities, and the proportions of its essential constituent parts, correspond with the age, sex, temperament, and habits of each individual in all respects; being modified by climate, season, and atmospherical vicissitudes; but the constitution of man has a wonderful power of adapting itself to external circumstances. It may also be affirmed, that to a considerable extent, not only the construction of the human physical frame, but the habits of mankind, both of action and thought, must be influenced, and in a considerable degree regulated, by the nature of his diet.

The Elements of Respiration maintain Animal Heat; the Elements of Nutrition maintain Vital Force.

Heat is essential to nutrition and to the manifestation of vital force: the fœtus in utero is preserved at the necessary temperature by the mother. In extra-uterine existence, if food be the first condition of life, oxygen is the second, and the combustion of the elements of the food supplies heat sufficient for the support of the vital force, and for the continuance of the vital processes.

According to Liebig, the following substances contained in our food are elements of respiration:—

Fat	Pectine.
Starch	Bassorine.
Gum	Wine.
Cane-sugar	Beer.
Grape-sugar	Spirits.

It has been explained in former papers, that by means of these substances a quantity of carbon and hydrogen is added to the nitrogenised compounds of our food, the latter compounds containing all the carbon and hydrogen which are necessary for the formation of blood, and the growth and reproduction of the tissues; hence the additional hydro-carbonaceous materials cannot be employed in blood-making; they are simply burned, and their combustion produces animal heat.

The carbon consumed by man in the protenised materials of his food and in the waste of his tissues, bears a very small proportion to the quantity of carbon given off from his system in union with oxygen,—a clear proof that in his case the carbon must be derived from some other source besides the waste or change of matter.

We find it the same in herbivorous animals. In the horse, for instance, the quantity of nitrogen ingested as food, which is about four ounces and a half, and the quantity of carbon associated with it, bears a very small proportion to the quantity of carbon expired daily, viz., seventy-nine ounces. So that rather less than a fifth part of the carbon employed in respiration is received into the system in combination with nitrogen; the remaining four-fifths must be derived from the starch and analogous principles of the food. If the animal were not supplied with these substances, his organism would be destroyed by the action of oxygen.

Liebig remarks, that if man, in such a community as the one we live in, subsisted entirely upon animal food, and was dependent upon flesh and blood for all the carbon which he requires in respiration, we should soon have a limit to the increase of population. Four pounds of starch is equivalent in carbon to fifteen pounds of flesh; so that if an individual could maintain himself a certain number of days upon a given quantity of flesh with an equal quantity of starch,

on being fed with flesh alone, during an equal period, he would require four times the quantity in order to obtain the carbon necessary for respiration.

Thus we have a clear conception of two distinct purposes for which food is required in our own systems—the maintenance of animal heat, and the nutrition of the various structures.

The constituents of the food must undergo various changes within the economy, before they are ultimately consumed by the oxygen of the atmosphere. Chemistry, until recently, had explained but very few of these changes. According to Liebig the protenised principles are formed into living tissue, and when again separated the greater part of their carbon and hydrogen is first converted into bile, and then into carbonic acid gas and water; their nitrogen, with the smaller part of their carbon and hydrogen, form the constituent principles of urine. Protein also forms gelatine and gelatinous tissues, and passes through the metamorphoses to which the constituents of these tissues are subject. The non-nitrogenised principles are in part deposited as fat; in part, also, they are converted into bile, and then into carbonic acid gas and water. But there is reason to believe that they are in part consumed by the atmospheric oxygen, after undergoing some unknown metamorphosis, or, at all events, without having been previously deposited as fat or converted into bile.

In the human body, then, the fuel for the production of animal heat is derived from the metamorphosis of the living tissues, from the bile, from fat, and from the hydro-carbonaceous materials of the food.

The imperfect state of our knowledge respecting the proximate constitution of the food we are in the daily use of, has been adverted to, and it has already been stated that the foundation on which the chemistry of living systems must be reared, is a knowledge of the ultimate constitution of all the materials which are received into and separated from the body (Paper IV., p. 390).

This view has been taken by Liebig; and with respect to the carbon, he has adopted the plan of determining the average quantity consumed by 27 to 30 soldiers in a month, or by 855 men for one day. The carbon contained in every article of diet being taken into consideration, the whole amounted to $13\frac{8}{10}$ ounces per man, daily. In several instances Liebig assumes this as a datum upon which his reasoning is founded, and as there are some objections to the system of averages in such an investigation, I determined to test the accuracy of his conclusions as applied to our customs in this country, by an examination of the ultimate composition of the diet actually consumed by one individual in twenty-four hours.

I have, moreover, extended the investigation, and attempted that which I believe has never been attempted before, viz., to determine the quantities of the ultimate elements of all the materials received into the system in a given period, and at the same time to give a rational account of the ultimate disposal of the equivalents of these materials in their elimination from the body. Boussingault has in part effected the same thing experimentally as respects the food and excretions of the horse and cow.—(Liebig, p. 290-1.)

The attempt can only be regarded as a rude one. I publish it as an exposition of what I believe to be the present aim of this branch of animal chemistry; it is comprised in the following tables, respecting which no one thing has been taken at a guess, but every quantity has been arrived at by calculations, founded on the analyses of animal substances contained in standard works on this branch of chemistry, and on the analyses of the articles of diet contained in Tables I. and II. (p. 384 to 391); accuracy has been attended to, but not to the extent of minute decimal quantities. Hoping better things from the rapid strides which animal chemistry is now making, I have no notion that these tables will be permanently applicable; nevertheless, they may be taken in some measure as a standard of comparison; at least I have found them useful as such in a manner I will presently describe.

On examining Table III. we find that solid and fluid ingesta, weighing in this case 96 ounces for twenty-four hours, is composed as follows; of

	Oz.	dwts.	grs.
Water	71	18	14
Dry material	22	15	7
Alcohol	1	6	3
Oz.	96	0	0

And, according to the elementary analyses, of,

	Oz.	dwts.	grs.
Water	71	18	14
Carbon	11	15	$2\frac{1}{2}$
Azote		17	$4\frac{1}{2}$
Hydrogen	1	17	$8\frac{1}{2}$
Oxygen	8	14	8
Earths and salts		17	$10\frac{1}{2}$
Oz.	96	0	0

If we add to this, 33 oz. 3 dwts. 4 grs., the estimated quantity of oxygen gas received into the system by every known channel, we obtain 129 ozs. 3 dwts. 4 grs., as the total amount of matter employed by, or in one way or another disposed of, in twenty-four hours.

Before any comments are made upon this calculation I submit the question,—the body returning to the same weight, how are these materials, or their equivalents, ultimately disposed of in the animal economy? Table IV. is the result of the attempt to answer this question.

TABLE III.—The proportion of water to dry material and the elementary composition of the aliment consumed in one day by a hearty adult male weighing 156 pounds avoirdupois, in good circumstances, and subject to active exertion; with the aggregate amount of each constituent element. To which is appended the estimated consumption of oxygen by the lungs, stomach and skin, in ozs. dwts. and grains.

				Water.			Dry matter.			Gas.			Carbon.			Azote.			Hydrogen.			Oxygen.			Salts, earths.		
ALIMENT.	Oz.	dwt.	gr.	Oz.	d.	g.	Oz.	d.	g.	Oz.	d.	g.	Oz.	d.	g.	Oz.	d.	g.	Oz.	d.	g.	Oz.	d.	g.	Oz.	d.	g.
Lean of meat	8	0	0	5	2	21	2	17	3	....			1	9	15	0	8	14	0	4	8	0	12	4	0	2	10
Fat of ditto	1	10	0		....		1	10	0	....			1	3	17		....		0	3	12	0	2	19		....	
Soup and gravy	6	0	0	5	10	17	0	9	7	....			0	5	2	0	1	12	0	0	14	0	2	3		....	
Potatoes (rather old)	6	0	0	4	6	10	1	13	14	....			0	14	19	0	0	10	0	1	23	0	14	18	0	1	16
Green vegetables (peas)	3	0	0	1	1	0	1	19	0	....			0	16	14	0	1	13	0	2	12	0	16	17	0	1	16
Salt (dried)	0	3	0		....		0	3	0	....				....			....			....		....		0	3	0
Batter-pudding	4	0	0	3	1	16	0	18	8	....			0	9	8	0	1	0	0	1	11	0	6	1	0	0	12
Ripe fruit	4	0	0	3	7	0	0	13	0	....			0	5	17	0	0	0½	0	0	18	0	6	12	0	0	0½
Bread (fermented)	14	0	0	5	12	0	8	8	0	....			3	14	14½	0	3	12	0	9	23½	3	14	2	0	5	20
Butter	1	7	0		....		1	7	0	....			0	17	18		....		0	4	14	0	4	16		....	
Sugar	2	0	0		....		2	0	0	....			0	17	0		....		0	2	12	1	0	12		....	
Tea	24	0	0	23	15	0	0	5	0	....			0	1	15	0	0	6	0	0	6	0	1	3	0	1	18
Milk	2	0	0	1	14	20	0	5	4	....			0	2	19	0	0	5	0	0	11	0	1	11	0	0	6
Porter	10	0	0	9	6	15	0	5	0	....			0	2	7	0	0	3	0	0	21	0	1	12	0	0	5
Alcohol of ditto 4.2 per cent. 8 dwts. 9 grs.		....			....			....		....			0	4	9		....		0	1	2	0	2	22		....	
Wine (port, sp. gr. 985.5)	4	0	0	3	0	11	0	1	19	....			0	0	14	0	0	1	0	0	5	0	0	20	0	0	3
Alcohol of ditto 22.2 per cent.		....			....			....		....			0	9	6		....		0	2	8	0	6	4		....	
Water	6	0	0	6	0	0		....		....				....			....			....			....			....	
	96	0	0	71	18	14	22	15	7	....			11	15	2½	0	17	4½	1	17	8½	8	14	8	0	17	10½
Alcohol to be added		....			....		1	6	3																		
OXYGEN GAS																											
Received by the lungs	32	10	0							32	10	0										32	10	0			
by stomach and cutis	0	13	4							0	13	4										0	13	4			
Ounces	129	3	4	71	18	14	24	1	10	33	3	4	11	15	2½	0	17	4½	1	17	8½	41	17	12	0	17	10½

TABLE IV.—The daily quantity of solid and liquid aliment being 96 ounces, as shown in Table III., the oxygen introduced by the lungs 32½ ounces, and the same element by the stomach, during digestion, or employed in the combustion of hydrogen or carbon on the cutaneous surface, 13 dwts. 4 grs. The whole = 129 ozs. 3 dwts. 4 grs.; the following Table exhibits the various modes in which the equivalents of the component parts of the ingesta are eliminated from the body; with the elementary constitution of the solid, fluid, and gaseous compounds into which they are distributed.

		Water.	Dry material.	Gas.	Carbon.	Azote.	Hydrogen.	Oxygen.	Salts, earths.
	Oz. dwt. gr.	Oz. d. g.	Oz. d. g.	Oz. d. g.	Oz. d. g.	Oz. d. g.	Oz. d. gr.	Oz. d. g.	Oz. d. g.
Fæces	5 10 0	4 2 12	1 7 12	....	0 12 12	0 0 12	0 1 21	0 9 0	0 3 15
Urine	42 0 0	39 3 17½	2 16 6½	....	....	....	....	....	....
Urea of ditto ....Oz. 1 5 5	....	....	....	....	0 5 2	0 11 19	0 1 16	0 6 16	....
Uric acid 0 0 20	....	....	....	....	0 0 7	0 0 7	0 0 1	0 0 5	....
Ammonia 0 2 5	....	....	....	....	....	0 1 19	0 0 10	....	....
Animal matters, including some phosphorus and sulphur 0 15 5	....	....	....	....	0 7 2½	0 1 4½	0 0 20	0 6 2	....
Salts, deducting ammonia, and some phosphorus and sulphur..... 0 12 19½	....	....	....	....	....	....	....	....	0 12 19½
Secretions of wax, tears, mucus, &c..	2 10 0	2 2 0	0 8 0	....	0 3 0	0 1 0	0 1 0	0 3 0	0 1 0
Sweat and cutaneous secretions.....	5 0 0	4 19 0	0 1 0						
Pulmonary vapour.....	9 0 0	8 19 5	....	....	....	....	....		
Animal matter of ditto 5 per cent..	....	....	....	0 0 19	0 0 7	0 0 5	0 0 2	0 0 5	....
Cutaneous vapour									
From combustion of hydrogen.....Oz. 14 1 1½	26 15 17	26 13 5	....	....	....	....	1 11 5½	12 9 20	....
From ordinary causes 12 12 3½									
Animal matter of ditto.....	....	....	....	0 2 12	0 1 3	0 0 10	0 0 5	0 0 18	....
Carbonic acid from lungs.....	28 0 0	....	....	28 0 0	7 10 0	....	....	20 10 0	....
Carbonic acid from ditto and skin...	10 7 11	....	....	10 7 11	2 15 17	....	....	7 11 18	....
Ounces.....	129 3 4	85 19 15½	4 12 18½	38 10 18	11 15 2½	0 17 4½	1 17 8½	41 17 12	0 17 10½

On examination of the latter table, it appears that the equivalents of the materials employed in the time specified, are eliminated from the system in the following forms and proportions:—

	Oz.	dwts.	grs.
Dry material	4	12	18½
Water	85	19	15½
Gas	38	10	18
Oz.	129	3	4

And, according to the elementary analysis, the egesta are composed of,

	Oz.	dwts.	grs.
Water introduced into the system	71	18	14
Ditto, from combustion of 1 oz. 11 dwts. 5½ grs. of hydrogen	14	1	1½
Carbonic acid gas from the combustion of 10oz. 5 dwts. 17 grs. of carbon	38	10	18
Carbon (Secretions and excretions.)	1	9	9½
Azote (Secretions and excretions.)		17	14½
Hydrogen (Secretions and excretions.)		6	3
Oxygen (Secretions and excretions.)	1	5	22
Earths & salts. (Secretions and excretions.)		17	10½
Oz.	129	3	4

Or of,

	Oz.	dwts.	grs.
Water	85	19	15½
Carbon	11	15	2½
Azote		17	4½
Hydrogen	1	17	8½
Oxygen	41	17	12
Earths and salts		17	10½
Oz.	129	3	4

Some explanation is necessary of the manner in which these results have been obtained.

Fresh meat contains, according to Bœckmann, 75 parts of water and 25 of dry matter; but in roasting, about 30 per cent. of the water of beef is dissipated, so that 70 parts of roast beef contain 25 parts of dry matter, and the composition of the dry material, as stated by the same author, is as follows:—

Carbon	51.89
Hydrogen	7.59
Nitrogen	15.05
Oxygen	21.24
Salts and earths	4.23
	100.00

With these data the component parts of the animal food consumed were easily calculated. More difficulty was experienced with respect to the bread, as I had only an analysis of black bread before me. I first ascertained that the bread used at my own table on dessication at about 280° Fah., lost 40 per cent. of water, the composition of the solid residue being calculated from Vauquelin's analysis of flour (Table I.), with allowances for changes which occur in the panary process, gave for 480 grains, or one ounce of bread,

			Water.	Carbon.	Azote.	Hydg.	Oxyg.	Salts, &c.
Water	192 grs.	=	192.	..	..	..	..	..
Gum	15 ..	=	..	6.4	..	1.	7.6	..
Starch	201 ..	=	..	90.	..	12.	99.	..
Sugar	24 ..	=	..	10.5	..	1.5	12.	..
Salts	10 ..	=	..	..	..	..	..	10.
Gluten	38 ..	=	..	21.*	6.	2.6	8.4	..
Grs.	480	=	192.	127.9	6.	17.1	127.	10.

The animal matter of soup and gravy were taken to be chiefly gelatine and osmazome. Potatoes, according to Boussingault, contain .34 per cent. of azote, a part of which they lose in keeping. The solid matter of porter consists chiefly of sugar and vegetable extractive, and that of wine, of extractive and tartar. It would serve no purpose to go further into these details, since they are cited only to show that I have neglected no circumstance of which I was aware to ensure an approximation to the truth.

I believe that the results have produced an aggregate amount of azote, which is in excess. Messrs. Dumas and Cahours, in a paper published in the "Gazette des Hôpitaux" for December, 1842, estimate the quantity of azote received into the system with the food in twenty-four hours, at about half an ounce. My estimate exceeds three-quarters of an ounce; but the gentlemen quoted do not give their data, and the absolute weight and full diet of the individual must be considered. Table III. shows that by far the greatest portion of the azote is derived from the bread and meat, and, of course, a corresponding increase must attend an increased consumption of these particular articles of diet. Still, I think, upon the whole, there is an excess of azote in my estimate. It

is highly probable that more oxygen is imbibed by the skin and less by the lungs than indicated; but here, as in several other instances, the principle of compensation applies.

Many remarks suggest themselves in reference to Table IV., one or two of which are necessary in this place. The fæces and the azote they contain are, perhaps, rather underrated. The urine is calculated, according to Berzelius's analysis, in which the animal and saline matter (regarding this analysis as an average), is undoubtedly in excess. Yet the sum of these materials in 42 ounces of the fluid corresponds in the case before us with the given diet. The estimated quantity of the carbonic acid gas given off may also, at first sight, appear to be excessive. The quantity of carbon excreted by the lungs has been stated by physiologists at from five to eleven ounces troy. No doubt the relative proportions of carbon excreted by the lungs and skin differ from those given in the table. All that can be said is, the diet being composed as indicated in Table III., the quantity of carbon converted into carbonic acid gas must be 10 oz. 5 dwts. 17 grs., or thereabouts. Comparative physiology, and observation and experiment on the human body, assure us that the skin and lungs perform a vicarious respiratory function as respects each other, and no conclusion can be arrived at as to these variable phenomena of life, except upon the principle of compensation.

Notwithstanding all their imperfections, these tables, to my mind, furnish something approaching a rational exposition, of the ultimate disposal of every particle of matter assimilated by the living human body, not only in a qualitative but in a quantitative point of view. Every reflecting reader will perceive in them certain self-evident truths; they also comprise numerous approximative, variable and compensating quantities. It is on account of these latter that it becomes so necessary for the medical philosopher to comprehend the human system as a whole, and this tabular plan, if it had no other utility, assists the mind in this respect. Those who have studied chemical physiology generally, and Liebig's work in particular, must have some definite notions respecting the intermediate changes which the materials represented in Table III. undergo in the living system in their passage to the forms of matter represented in Table IV. How trifling is the advance which this branch of knowledge has yet made it is not necessary for me to demonstrate; how much we are indebted to Liebig for pointing out the right method of proceeding to ensure its rapid progress, has, I think, become evident in these papers.

It will be a question with the reader, how far the method I have pursued in working out this complicated problem substantiates Liebig's views. In all essential particulars it does so completely; there must be a difference in the details, but this is obviously within such a range that it does not interfere with the general conclusions.

Although Liebig gives $13\frac{2}{10}$ ozs. as the average daily consumption of carbon by his German soldiers, yet he found the average in a particular family, consisting of nine persons of different ages, not more than $9\frac{1}{4}$ ozs. The subject of my investigation consumed $11\frac{1}{4}$ ozs. of carbon as food, of which $10\frac{1}{4}$ ozs. must have been burned and converted into carbonic acid gas; more than $1\frac{1}{2}$ oz. of hydrogen must also have been burned. Now, the combustion of this $11\frac{3}{4}$ ozs. of carbon and $1\frac{1}{2}$ oz. of hydrogen, would produce sufficient heat to maintain the animal temperature of the individual for the twenty-four hours, calculating according to the data furnished by Despretz, and taking all the concomitant circumstances into consideration. Neither is there anything in the case analysed in the construction of these tables, which militates against Liebig's theory of the animal matter of the urine being the product of the metamorphoses of the tissues; on the contrary, everything is corroborative thereof.

A Knowledge of the Nutritive Value of the Proximate Principles of which Food is composed,

is of daily application. I may exemplify this by the not infrequent case of an attempt to nourish an infant just weaned with a diet consisting almost entirely of starch. We know well the consequences. Experience had taught us the remedy, and theory now explains most satisfactorily the *modus operandi* of that remedy. The component parts of the animal food consumed were easily calculated upon these data. But a knowledge of the proportions of the *ultimate elements* of matter received into the system as food, has also, we now find, a real importance, and this it is which has been entirely overlooked. I have made Table III. practically useful as a standard of comparison. A legal gentleman, of active mental and bodily habits, consulted me with symptoms of general debility, accompanied by those which I believed to indicate a continued state of congestion of the liver. He furnished me with a fair average statement of the whole of his daily diet. Upon examining this table accurately, comparing it with that now published, and making certain allowances for a different constitution and different habits, I was enabled to conclude, with a great degree of certainty, that the quantity of water was in excess, that of dry material defective, and that it contained too great a proportion of hydro-carbonaceous matter, and too small a quantity of azote. A regulation of the diet, according to these principles, lessening the proportion of watery fluid, of fat, butter, and other carbonaceous materials, and increasing the proportion of good wheaten

bread and meat, has been followed by beneficial results.

Another individual, of regular and what are called moderate habits, for many years affected with *acne rosacea* and various dyspeptic ailments, who has had one severe attack of rheumatic fever and been threatened with many, furnished me with a diet table, in which the daily consumption was found to be 120 ounces. Examining this closely by the same reasoning as Table III. was constructed by, I found an excess of dry material as compared with water, an excess of hydro-carbon in the form of alcohol, and a great excess of azote and of its compounds as compared with all the other principles of the food. Modifying his diet in reference to these errors has been followed again by the very best effects; the rheumatic tendency is quite gone, and the tumefaction and redness of the face is less than for many years past.

I have before me at the present moment the dietaries of two octogenarians; one being an old officer, aged 82, and the other a retired tradesman, aged 81; the former takes no more than 40 ounces both solid and liquid, in twenty-four hours; the latter takes 63 ounces, both being individuals of regular habits of life. The diet agrees in the circumstance of the very small quantity of azotised food which they are capable of consuming being consistent with their time of life. But in another instance the quantity and relative proportions of water and dry material consumed by an individual aged 42, was found to be 12½ ounces, upon an average, daily, to 73 ounces of water; a diet, I should say, totally inconsistent with vigour of constitution and continued good health at that age. To extend these remarks would carry us beyond the objects of the present undertaking; I will only say that I am convinced this is the method by which the problems relating to diet will be ultimately solved.

If Liebig's principles can be thus applied to the investigation of the habits of individuals in private life, with reference to their highly-complicated diet, how much more readily and more advantageously might they be resorted to in public institutions, where the diet is more simple, where averages may be drawn on a large scale, where the food actually consumed might be analysed with facility, and where the effects might be vigilantly watched and noted. I throw this out as a suggestion, which I hope soon to see acted upon generally, convinced as I am of its importance, and of the beneficial results which it would lead to. The comparative results of different tables in different institutions would increase the utility of this mode of investigation.

CHLOROSIS SIMULATING PHTHISIS.

A CASE of this disease, simulating the last stage of phthisis is reported in the "Bulletin Med. de Bordeaux" by Dr. Chabrely:—M. O., a female, eighteen years of age, in a state of complete marasmus, had kept her bed for the previous three months, with hectic symptoms of great intensity. She had been at first attacked by symptoms like those of acute gastro-enteritis, but against which general and local bleedings were employed without success. The patient had been ever since kept on low diet. Dr. C. found her suffering from difficult digestion; and ascertained that the menses, which had seldom appeared, had altogether ceased since the access of her present attack. Considering this the primary evil, the efforts of the physician were first directed to its removal, for which end he enjoined nourishing diet, and ordered lozenges of lactate of iron, and a few drops of tinct. digitalis night and morning, together with the employment of dry cupping (des grandes ventouses) over the hip region, every two or three days. After the eighth time of application of the latter, the patient began to experience a sense of weight in the pelvis, and at about the fifteenth application a white discharge from the vagina was seen, which increased daily in quantity. The appetite, sleep, and general health now improved rapidly; the menses soon reappeared; and after about six weeks of this treatment the cure was complete.

The editor of the "Gazette Medicale," in commenting on this case, considers the recovery to have been mainly owing to the adoption of a generous diet; Dr. Chabrely attributes it mostly to the cupping. It may have been due to neither so much as to the lactate of iron.

BATHS OF BICHLORIDE OF MERCURY.

WEDEKIND has advocated such baths for chronic rheumatism and sciatica, Ebel in cases of arthritis, and Trusen against paralysis of the lower extremities, in the removal of which he has seen it uniformly efficacious after a variable lapse of time. He advises a bath at a temperature of 90° to 95° Fah., medicated with the bichloride, and in which the patient should remain for half an hour daily, perspiration being kept up after coming out of the bath by remaining in bed for a few hours, and taking warm drinks. Dr. Trusen states that he has never witnessed untoward symptoms from the use of the corrosive sublimate thus employed; only in one case a moderate salivation supervened after the use of the fortieth bath.—*Hufeland's Journal.*

EMPLOYMENT OF ELECTRO-MAGNETISM IN A CASE OF POISONING,

WITH SUGGESTIONS FOR ITS APPLICATION TO STILL-BORN CHILDREN AND SOME FORMS OF DISEASE.

By THOMAS S. PAGE, M.D., of Valparaiso.
(Communicated to THE LANCET by JOHN HOGG, Esq., Surgeon, Gower-street, London.*)

A. T., an Englishman, the subject of this communication, aged 22 years, and of robust frame, is a clerk in one of the most respectable commercial houses in Valparaiso. He had a slight gleet, for which he prescribed himself pulverised cubebs, in doses of half an ounce, night and morning, and experienced from them neither good nor bad effects. On the night of the 16th of March, 1842, he went to an apothecary's shop and asked for cubebs. Not having confidence in the lad in attendance, he requested permission to examine the label on the bottle, and read thereon "Pulv. Cubeb." He then ordered an ounce, divided into two parts, and with these returned home at midnight. He immediately took one of the powders, placed himself in bed, and, as was his custom, took up a book to read, but, as he expresses it, had not read two lines before he felt a dizziness and inclination to sleep. I accidentally discovered him the following morning about twelve o'clock with these symptoms: face red and swollen; lips dark-purple; mouth containing a viscid frothy saliva; tongue has a dry and chapped appearance in the centre, and the teeth are slightly coated with a brown sordes; veins of the forehead and temples turgid; eyes rolled upwards, injected, and their pupils contracted to a point; skin moderately warm and moist, with clammy perspiration; feet cool; pulse very slow, moderately full, and dispersed by the least pressure; respiration very slow, short, and gasping. By agitating him violently he was aroused for a moment, uttered some incoherent expressions, and sank back into comatose sleep.

These were the symptoms when I first saw him. Dr. Houstoun of the Royal Navy, now practising in this place, and Dr. Barrabino, of the U. S. Navy, then attached to the U. S. schooner, *Shark*, came to my assistance. We administered the sulphate of zinc as an emetic, and hot mustard and water to arouse the sensibilities of the stomach to its impression. Large draughts of this, and titillation of the fauces, produced vomiting, and a small quantity of the powder apparently was brought up. The stomach-pump was at hand, but as vomiting was readily provoked, it was not used. The patient was made to sit on the edge of the bed with his feet hanging in a tub of almost boiling water strongly charged with mustard. One cup was applied to each temple, and about two ounces of blood abstracted. Large sinapisms were spread over the chest and stomach and inner parts of the thighs. A very strong liniment of ammonia, cantharides, and turpentine, was applied to the whole length of the spinal column, until the skin became very red and inflamed. When the stomach seemed to be cleared of all traces of the poison, the mustard draught was suspended, and a large quantity of olive, with castor oil, administered, but only a part remained. The patient now appeared to be sinking. The surface was cold and covered with a damp sweat; the face was pallid, with a purplish tinge; the jaw and eyelids were fallen, which the patient, by powerful sternutatories and severe blows on the face and shoulders with the open hand, was with difficulty made to raise. Ammonia and brandy and water were now given, with light broths, and an injection composed of turpentine and ammonia. This produced a slight discharge from the bowels. The stimulating liniment already mentioned was repeated to the spine and over the surface of the body. The pulse was hardly perceptible at the wrists, if, at times, it was at all to be felt. The stimulants were continued.

It was now 3 p.m. There were no signs of reaction, and the features wore the aspect of death. Under these discouraging circumstances, and when every effort seemed vainly expended, we now determined to dress the patient and, supported by two strong assistants, to take him from his room, continue the stimulants and light broths, and endeavour to walk him in the cool air. At first he made feeble but unsuccessful efforts to direct the movements of his legs, but at length could not be aroused, made no effort to stand, and sank almost lifeless into the

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* *To the Editor of* THE LANCET.

Sir,—The following cases and observations having been forwarded to me from South America, by Dr. Page, I send them to you in the hope that they may prove interesting to your numerous readers. I am acquainted with Mr. A. T., the subject of the first case, and had heard of his narrow escape from his friends here. I may also add that a portion of the cubebs has been sent over, and has been examined by two eminent pharmaceutical chemists without their being able to detect any deleterious drug in combination with it, so that the effects described may be reasonably attributed to the magnitude of the doses. I am, Sir, your obedient servant,

JOHN HOGG.

Gower-street, Bedford-square,
Jan. 24, 1843.

arms of his assistants.* He was carried to his room and placed in a slightly reclining posture on his bed. His breathing was now short and hurried; his mouth wide extended and jaw fallen; nothing seemed capable of arousing him; the exhaustion was extreme; the pulse could be felt feebly at the wrist, maintained there probably by the agitation which he had just undergone. Dr. Houstoun had left a short time previous, Dr. Barrabino remained with me.

It was now 4 p.m., and worn out with fruitless efforts we desisted entirely from further exertion. At this conjuncture I thought of my electro-magnetic battery, and proposed its application to bring about reaction, for I felt we were justified in such desponding circumstances to make it a matter of experiment. Cerebral congestion was urged as an objection, but admitted not to be sufficient, in such a desperate case, to set aside the experiment. It was immediately tried, and with the happiest results. With an assistant rapidly rotating the wheel, I applied the balls at first to each side of the neck, and ran them down behind the clavicles. The arms and body now moved convulsively, but the patient lay as unconscious as before. I now passed one ball over the region of the heart, and the other to a corresponding point on the right side. In an instant his eyes opened widely, and with a ghastly expression of countenance; his head and body were thrown convulsively toward me, and he groaned. He now sank back into his reclining posture, and he was again asleep. The balls were re-applied in the same situation with similar results—a third and fourth time, and he cried, "No more." Reaction was now positively established; the heart had received a strong impulse; the pulse was becoming rapidly developed, and the whole surface warm.

We now determined to desist, and watching him attentively, allow him to remain quiet for an hour. Reaction continued satisfactorily, and when the hour had expired he could be awaked by shaking his body and calling loudly his name. There was no further occasion for the battery. He was aroused at intervals; and at eleven o'clock in the evening was sufficiently awake to relate where he had got the medicine the preceding night, but was still drowsy, and when not disturbed inclined to sleep. Thus he passed the night, and on the following morning was pretty well. He then told me that he heard many things the preceding day that were said by persons about him, but he neither felt the power to open his eyes nor move his tongue to speak, although up to 3 p.m., when powerfully agitated and spoken to, he would reply in short and sometimes broken sentences, and occasionally correctly. He further says that the last thing he has any recollection of was my remark, whilst they were attempting to walk him in the corridor, that nothing more could be done but to make the experiment. From that time all was blank to him, until, as he expressed it, "he felt as if a gun had been fired off within him, which thrilled through and shook him to the very extremities." This was the application and effect of the electro-magnetic battery.

I have said that cerebral congestion was thought at the time to make the application of the battery in such cases objectionable. The result proved the incorrectness of this opinion, and sustains this argument in favour of the practice adopted, viz.,—By observing the phenomena of diseases a relation may be remarked between some of them in their earlier stages, whose terminations and consequences are quite dissimilar; apoplexy and epilepsy furnish an example. In both there is great cerebral congestion. The former generally terminates in effusion, and paralysis is the consequence; the latter terminates in spasms, and the patient returns to his usual health. Therefore it would seem that the muscular spasms equalise the circulation, and thus unload the brain; or, if we might suppose epilepsy to depend upon a determination, if I may use the expression, of the nervous principle to the nervous centres, the latter are relieved by throwing it off upon the nervous extremities, occasioning thereby spasm. Viewing these vital actions as the efforts of nature to relieve organs from the effects of undue accumulations, and restore the equilibrium in the nervous and vascular systems, it appears probable that severe narcotism of the nervous centres may be diffused and shaken off by the revulsive action of the battery on the nervous branches, and that the consequent developments of vital action would give an impulse to the general circulation which might relieve the cerebral congestion.

A question might arise as to the power of the medicine taken to destroy life. On this point the melancholy death of Mr. C., a French gentleman, late of this place, who took the same medicine and in the same quantity but a few weeks previous, affords convincing testimony. In illustration of my subject I asked Dr. Cazentre, a French practitioner of this place, and the physician of Mr. C., for an account of the circumstances attending his death, and the autopsy, which he very politely communicated as follows:—

"Mr. C. was afflicted for some time with a gonorrhœa, for which, without medical advice, he took balsam copaiba, even in large doses, but to no effect. He was now attacked with orchitis, and for the first time came to consult me. After eight days of constant attendance the swelling which existed in the left testicle disappeared, but the

* Broths and stimulants were poured into his mouth, but he could no longer swallow them.

blenorrhagia returned with more force. Vexed with this he again wished to take remedies which might relieve him at once of this afflicting disease. I recommended the use of the cubebs, which, taken for nine days, and gradually augmented to two drachms three times a-day, had almost completely taken away this obstinate affection, when, on the 13th of February, not having any of the remedy left, he sent the same recipe to the nearest apothecary's shop.* At ten o'clock at night he retired to his bed-chamber well and cheerful. Without consulting any one, on lying down, he took half an ounce of cubebs. No noise whatever was heard during the night, and at seven o'clock the following morning, when they entered his bed-chamber, they found him in a state of insensibility. Half an hour after I was with him, assisted by Dr. Veillon, and we found him in the following condition :—

"The body is in a state of supination; all the senses are extinguished, without hearing, speech, or movement; the eyelids are fallen, and when raised the eyes look cloudy and fixed; the pupils are dilated; extremities flexible; they obey the hand which raises them, and fall like an inert body; heat natural and equally diffused; face red; there are coloured, blackish spots on various parts of the body, but principally on the back; when the body is moved a species of strong râle is heard in the bronchia; pulse slow, feeble, and very irregular; respiration hardly perceptible. Not knowing to what to attribute a state so suddenly produced and so grave, and recognising by the symptoms the appearance of asphyxia, and thinking he might have taken too strong a dose of the medicine, Dr. Veillon and myself proceeded in consequence to extract from the stomach the cubeb that it might yet contain, and to cause reaction by the most powerful excitants. But all was in vain, and at twelve (noon) life was completely extinct. Nothing now remained but to make the autopsy, which we did the following morning at seven o'clock. All the cavities and organs were examined with the greatest care.

"*Exterior.*—The face is pallid, the integuments livid, principally behind, and the corpse rigid.

"*Abdomen.*—The stomach with no trace of inflammation, contains a tumblerful of liquid, in which is observed a little of the powdered cubebs, mixed with some aliment, which is almost digested; the intestines are sound and healthy; the bladder is full of crystalline urine; the liver, spleen, and kidneys are full of black and fluid blood.

"*Thorax.*—The lungs are excessively engorged with blood, and when cut into with the knife this flows with a great abundance of froth from the bronchia; the left side of the heart is entirely empty, the right full of blood; the aorta and all the arterial system is entirely empty; the venous system of the thorax and abdomen, as well as the pulmonary artery, the vena cava and portæ, are full of black and fluid blood, which flows abundantly as soon as the vessels are divided.

"*Head.*—The veins of the brain are also congested, but the congestion of this organ is not as great as that observed in the thoracic and abdominal viscera. I repeat, that in no part was there red or congulated blood found, but always black and fluid, and filling all that appertained to the venous system."

Before closing this subject, I would beg leave to add my impression that electro-magnetism will not only be found a most useful agent in cases like the above, but in some forms of disease, particularly those of a highly congestive character, where oppression of the organs and the nervous system prevent reaction and speedily destroy life. I need not occupy space in adducing cases illustrative of my meaning. In practice I think we frequently see cases where death seems to be caused by an *obstruction* of the functions or organic movements which support life, more than by an *exhaustion* of the organic functions or of life itself. And in such cases electro-magnetism might communicate an impulse which would renew those sympathetic actions between the organs (if no positive lesion exist in any of them) upon which the continuance of life depends.

In all cases of asphyxia, electro-magnetism must be useful; and I am strongly impressed with the belief that it might be applied in very many instances to still-born children with the happiest effects; for this purpose an instrument might be used of a very portable form,—that used by me in the case related consists of a large *horse-shoe magnet*, mounted upon a stand, in a vertical position, with an *armature*, fixed upon an axis between the poles, so as to revolve in front by means of a wheel. The *armature* consists of two cylindrical bars of soft iron, connected by a cross-bar, and in the *centre* of the bar is an insulated ferule; as near the end of the bar as possible is fastened the "*breakpiece.*" Around each of the cylindrical bars is wound *two thousand two hundred and fifty feet of wire*, covered with cotton thread, to prevent the current of electricity from passing from one wire to the other; the end of *one* of the coils is connected with the "*breakpiece,*" and the other with the *ferule.* From one of the pillars, which are in *front* of the armature, the *springs* are made to act on the ferule and breakpiece. From the other pillar the spring connects with the *centre;* the *handles* are fastened by the *set screws* in

* He had previously got the cubebs at another shop, but on this occasion purchased it at the same shop where my patient subsequently procured his.

the base of the pillars. There are four set screws in the back of the upright block of wood to set out the magnet, so as to make the armature revolve as close as possible to the magnet. The shock is communicated on ordinary occasions by grasping large brass handles which connect with the instrument by short coils of wire, which are painted red. To apply it to different parts of the body long wires, covered with cotton-thread, and terminating in brass balls, are used. Two glass cylinders enclose the wire near the balls, for the operator to hold by while administering the shock.

ON THE

COMPOSITION OF PURE AND VITIATED ATMOSPHERES.

By R. D. Thomson, M.D., Glasgow.

Strange as it may appear, the composition of common air is not, even yet, satisfactorily determined, in the opinions of all chemists. According to Dumas, "Ann. de Chim.," iii., 267, the atmosphere consists of oxygen, by weight, 23., azote, 77., and, dividing these numbers by what he has found to be the specific gravities of the gases, he deduces the composition in bulk to be

$\frac{23}{1.1057} = 20.80$ oxygen, $+ \frac{97}{972} = 79.22$. Azote.

Calculation, however, shows that the specific gravity of oxygen, according to his data, ought to be 1.1066 or 1.1067; and we believe Dumas considers that the true specific gravity cannot be under 1.107. This is very near Dr. Thomson's number, viz., 1.1111.

Dumas has lately published some experiments, made at Copenhagen, on air taken from the surface of the ocean, where the ratios of the gases vary considerably from these data as follows:—

	By weight.	By Bulk. S. g. 1.1111.	By Bulk. S. g. 1.1067.
Oxygen	22.58	20.3	20.40
Azote	77.42	79.7	79.60

It deserves remark, that the mean of six experiments out of ten, made by Dr. Thomas Thomson (First Principles, i., 98), gave for the composition of air at Glasgow, by bulk—

Oxygen	20.42
Azote	79.58

This remarkable coincidence between the composition of the air at the sea, where vegetation is absent, and of that in the oxygen-consuming city, perhaps deserves more attention than has yet been paid to it.

In connection with experiments upon pure air, the trials of Leblanc upon vitiated atmospheres are of high interest. The quantity of carbonic acid in the atmosphere in the normal state has been shown by the Saussures to vary from 3 to 6 parts in 10,000. Leblanc (Ann. de Chim., v., 223,) has examined the quantity in crowded rooms, theatres, cities, &c. In the hospital La Pitie, the air of one of the wards containing 54 patients, afforded $\frac{1}{1000}$ of CO_2, or 5 times more than that of normal air. Under similar circumstances, at the Salpetriere, the quantity was $\frac{8}{1000}$. In Dumas' class-room, after a lecture of an hour and a half, where nine hundred persons were present, the carbonic acid amounted to 1 per cent., and the same quantity of oxygen had disappeared. From other experiments, he considers this a maximum quantity for safety, and strongly recommends a better ventilation when so much carbonic acid is present. This result agrees with experiments made in this country. When the atmosphere is deteriorated by burning charcoal, he has seen death produced when 3 per cent. of carbonic acid was present in the atmosphere. In all such cases of death from stoves, he has found carbonic oxide in the air, and he attributes a deleterious effect to the agency of this gas. He has observed 1 per cent. of this gas to destroy an animal in two minutes, which is at variance with the statement of Nysten. This observation explains many of the inconsistencies which appeared some years ago in the evidence of some London chemists respecting the influence of Joyce's stoves. It is quite obvious that their structure was dangerous. Leblanc found that a candle was extinguished in air containing 4½ or 6 per cent. of carbonic acid. In such an atmosphere life may be kept up for some time, but respiration is oppressive, and the animal is affected with very great uneasiness. Air expired from the lungs contains about 4 per cent. of carbonic acid, and hence this atmosphere is noxious. Even 3 per cent. in the atmosphere killed birds, and yet we have seen statements which affirmed that upwards of 3 per cent. had been detected in the London theatres. All these facts are pregnant with importance in reference to health. Our miners may not be suffocated by fire-damp explosions, but we should remember that their constitutions may be poisoned by the respiration of tainted atmospheres.—*Proceedings of the Glasgow Philosophical Society*, No. 4.

The Talk all on One Side.—Desgenettes had a way peculiarly his own of conducting his inquiries respecting his patients' ailments. He would put a question, but never suffer an answer to be made. If the patient offered to speak, the professor would say, "Your interruption is neither polite nor politic; it is rude to interrupt any one who is speaking to you; and it is unwise, because while am talking all the time that I can spare elapses."

ENCYSTED ABSCESS OF THE BRAIN.

To the Editor of THE LANCET.

SIR,—The following case, having lately occurred in my practice, which I deem of some interest in the class of diseases to which it belongs, I should feel obliged by your giving it insertion in one of your forthcoming numbers. I remain, Sir, your most obedient servant,

DAVID BOLTON,
Demonstrator of Anatomy at the School of Medicine and Surgery, Birmingham.

Jan. 1843.

G. S., by trade a bookbinder, had been suffering at intervals, during the last five years, from pulmonary disorder, to which there appeared a constitutional predisposition. In the beginning of November last he applied to me with an attack of his former complaint, and remained under treatment for a period of five weeks, during which time there were no symptoms of cerebral disturbance. On the subsidence of the pulmonary affection he complained of pain and giddiness in the head, which he referred to the temporal and frontal regions, and I was disposed to ascribe these symptoms to congestion, arising, probably, from the previous pulmonary disturbance. He attended at my house every second or third day for about a fortnight, and as the urgency of his symptoms did not seem to demand very active treatment, his case was treated by local depletion and occasional purgatives, which merely afforded temporary relief. As the pain became more fixed and aggravated, and attended with great irritability of the stomach, it was evident that some serious changes were going on in the head. Depletion, counter-irritation, with alterative doses of mercury, were now had recourse to, and carried as far as practicable, but without any apparent benefit. During the last week of his illness he was attended at his own house, and visited daily, but not confined to his bed. On my latter visits I found him still complaining of pain in his head; the pulse was quiet; the irritability of the stomach had subsided; and he answered questions distinctly and coherently. There was (with the exception of a little confusion of vision) an absence of all muscular and sensorial disturbance, and there were no general signs to warrant the belief that extensive disorganisation was going on in the head. On the morning of his death he was giving his wife directions to remove some source of annoyance to his feelings, when he suddenly fell back and became comatose; in this state he remained till the evening, when he died.

Sectio Cadaveris.—On raising the skull the membranes were found slightly injected, the cerebral veins were distended, and there was an evident protuberance on the posterior part of the right hemisphere, which at first sight appeared like a tumour protruding through the cerebral structure. On attempting to remove the brain from the skull, about an ounce and a half of offensive purulent fluid escaped from the right hemisphere, which on examination presented extensive disease. A cyst of considerable thickness occupied the centre of its posterior part, which had ruptured and allowed of extensive infiltration of its contents into the cerebral matter. The brain for some distance surrounding the cyst was broken up, and from being mixed with the purulent matter was of a creamy consistence, and presented a greenish-yellow appearance. The softening occupied the whole of the posterior and part of the middle lobe, as far as its inferior cornu. The ventricles contained about three drachms of greenish serum. The cyst itself was of the size of a large hen's egg; its walls were of variable thickness, being a quarter of an inch in its thickest part, and composed of three structures; the outer portion was made up of condensed cerebral structure, whilst the middle and inner layers presented a cellulo-vascular appearance; the lining membrane was coated with a layer of pus, on removing which numerous small vessels were seen ramifying on its surface. On examining the posterior part of the cyst a rent was discovered, which had permitted of the escape of its contents, and produced the suddenly fatal symptoms which terminated his existence.

Remarks.—The foregoing case presents some very remarkable features, inasmuch as it manifested, till the bursting of the abscess, little or no impairment of the sensorial or muscular functions. Dr. Abercrombie, with other eminent pathologists, have usually found encysted abscess of the brain to be accompanied with convulsions or paralysis, one or other of these affections being a common attendant; these, however, do not appear to be uniform symptoms. Broussais found an extensive abscess in the centre of each hemisphere, without any other symptoms than a peculiar dulness of manner, with taciturnity, which terminated in coma, after thirty-seven days. Morgagni found one in the posterior part of the brain, in a man who died of gangrene of the nates, without any cerebral symptoms marking its existence. Other pathologists have recorded cases of a similar description, and the present one presents a large abscess, with extensive lesion of the brain, with few symptoms to indicate its nature and existence. Pain of the head for a period of three weeks, with occasional sickness, were the only striking phenomena, and though the disease occupied the posterior part of the hemisphere, the pain was constantly referred to the temples and forehead. Judging from the toughness and

thickness of the cyst it would appear to have been of slow formation, yet the history of the symptoms would lead us to an opposite conclusion. On minute inquiry into his previous habits and state of health, he seemed to have suffered occasionally from slight pain of the head, which he attributed to disorder of the stomach, and was of short duration; neither did he appear to have had at any former period convulsions or fits of any description, and up to the three weeks preceding his death he had followed his usual occupation as a bookbinder. The question then arises, Was the abscess the result of insidious inflammation of the brain, supervening on the development of a scrofulous tumour, without any manifestations during life of the existence of the latter morbid product? I am inclined to take this view of the case, and have no doubt, if an opportunity had been afforded of examining the chest, that similar morbid deposits would have been found in the lungs. He had suffered for several years from pulmonary irritation, and being of a strumous habit of body it is not improbable that the cerebral disease had been called into action by mental anxiety from which he had recently suffered, combined with the recent pulmonary attack. This conjecture is suggested by the known changes which scrofulous tumours undergo in other situations of the body, which, after remaining dormant for some time, their existence unsuspected, from some exciting or determining cause rapidly advance to suppuration. The encysted abscess of the brain is usually considered by pathologists to be of gradual and slow formation, and often connected with the scrofulous diathesis, or an unhealthy habit of body; whilst the abscess consequent upon acute cerebritis has no defined cyst, the matter being circumscribed by the condensation of the surrounding cerebral substance. Admitting the abscess to have been of slow formation, it is remarkable that so serious and extensive disorganisation should have been going on in the surrounding structure with so few signs of its existence, and probably the slow but progressive development of this description of disease may account, in some measure, for the want of the ordinary characteristics of cerebral inflammation. The little impairment of the intellectual functions would seem to favour the conclusion, that when one hemisphere is extensively diseased the other is found sufficient to maintain the integrity of these powers, though this opinion is weakened by the general phenomena of cerebral affections. The anomalies attending this and similar cerebral lesions are only to be explained by an attentive and patient investigation of a series of this class of diseases; and on looking to the history, progress, and termination of the present case, we are impressed both with the difficulty in establishing a correct diagnosis of encysted abscess of the brain, and that fatal lesion may take place in its structure unattended by any very prominent symptoms of its existence.

From this, and other cases on record, we must conclude that the phenomena attendant on the formation and presence of matter in the brain may be variable; that we have no uniform train of symptoms declaring its seat and presence; and that a slow and dangerous disease may proceed to fatal coma, unattended by convulsions or paralysis.

SECTION OF THE PERINEUM TO PREVENT ITS RUPTURE,

WITH REMARKS ON THE SUBJECT.

By CHARLES MACKIN, M.D., Thame.

On the night of the 16th Nov. I received a summons to attend Mrs. R., ætat. 40 and upwards, in labour with her first child. On arriving, and making an examination per vaginam I found the vulva placed much higher towards the pubis than usual, the perineum very long, the posterior commissure of the labia considerably advanced forwards and upwards, so that the passage formed a segment of a circle round the pubis, the finger having to be curved round that bone, instead of being introduced, as in the normal conformation, in a direction nearly horizontal. The pains were weak, and the patient of exceedingly rigid and unyielding fibre. When the head had advanced so far as to render the perineum tense during a pain, I felt the impulse given by each contraction of the uterus to be directed wholly against the perineum, which by its resiliency constantly counteracted each expulsive effort, so that after a considerable period not the slightest progress was being made towards the birth of the head. However, I resolved to await the chance of natural dilatation of the parts, which I expected would, though tardily, at length take place, and render any interference unnecessary.

After waiting patiently for this "consummation most devoutly to be wished," and finding the head not advanced in the slightest degree, the external orifice no further dilated, or likely to be so, the perineum extremely thin during a pain, while the fourchette at its anterior margin was fully half an inch in thickness, gradually becoming more attenuated towards the centre of the perineum, through which every succeeding pain seemed likely to force the head, I balanced in my mind the two remedies which presented themselves, namely, venesection or section of the perineum. The patient being much exhausted, and the pains having been, from the beginning, much feebler than usual in a first labour, besides doubting whether bleeding was of the same service in rigidity of the external parts as in that of the os uteri,

the rigidity in this case being only a symptom superadded to the abnormal conformation, over which (of course) loss of blood could exert no possible influence, I decided on making section of the perineum to a sufficient extent to allow of the passage of the head, deeming that *in any case* the perineum must be divided either by laceration or incision before the head could pass. I therefore passed a director upon the head of the child, and on it divided the perineum to the extent of about an inch and a quarter (it being stretched over the cranium). On the next pain the head was expelled. The incision, on relaxation after the birth of the child taking place, was about two-thirds of an inch in extent, and has since united in its whole extent, so as to leave not the slightest inconvenience to the patient.

One of the earliest notices of this operation is to be found in THE LANCET of Nov. 26, 1836. It strikes me that it has by no means received the attention it merits as a means of facilitating the passage of the child in certain cases, and as a remedial measure in threatened laceration of the fourchette, or perforation of the perineum (the posterior commissure of the labia remaining intact), which latter accident we know sometimes happens, and I am inclined to think is a much more frequent occurrence than generally supposed. We know, likewise, that in spite of all the skill and address of the accoucheur, laceration to a greater or less extent will not unfrequently take place, of which we have numerous examples on record.

Laceration (to a slight extent) of the frœnum or posterior commissure of the labia is an almost invariable consequence of the birth of a first child, in a healthy female, and is considered such a usual concomitant that the absence of the frœnum is usually held to be a corroborative proof of former deliveries.

The extent of this laceration may depend on several causes, as for instance inefficient or ill-applied support during the passage of the child; unusual extent of the head, either in the antero-posterior or lateral diameters; the knees being separated too widely by placing a pillow between them; indeed, this last proceeding is a frequent cause, by putting the perineum more or less on the stretch, in addition to the strong distending force exercised on it by the head in its descent; and also may be caused by the vulva being placed higher up towards the pubis than usual, in which case the perineum is longer, and the coccyx and lower end of the sacrum are said to be curved more forwards; indeed the latter is stated by a modern author to be always found in cases where this vicious conformation is present. The sacro-vertebral angle, by being more salient than is commonly found in the well-formed female pelvis, may, by giving to the axis of the outlet a direction more downwards and backwards, thus diminishing the inclination of the plane which directs the head of the child from behind forwards and from above downwards, beneath the symphysis pubis, cause the expulsive force of the uterus to act in a line so much posterior to the vulva that the effect of each contraction is totally expended against the perineum; and this, I think, may be added to those already systematically laid down as the causes productive of tedious labour. Those who have attended first labours, when the vulva dilates slowly and with difficulty, the perineum, on the contrary, having a tendency to stretch and attenuate itself from the pressure of the child during a pain, must have been often struck by the extreme possibility of the presenting part being forced through. I believe, with Dupuytren, that this accident (perforation of the perineum) happens more commonly than generally supposed, save that in the majority of instances the commissure is likewise included in the laceration, and the case takes the name of rupture of the fourchette.

In the mal-conformation which I have described we can easily conceive the obstacles which the state of the parts would oppose to parturition. The head of the child has considerable difficulty in accomplishing its passage through the inferior outlet, the efforts of the uterus being directed against a point of the perineum so much posterior to the vulva, and, if the frœnum offers a proportionally lesser degree of resistance than the perineum, the infant passes through, indeed, but not without causing a laceration of more or less extent, which the accoucheur, whatever be his care, cannot prevent. If, on the contrary, the commissure be extremely rigid and resisting, the perineum gives way and the birth takes place through the opening, the fourchette remaining whole. It appears somewhat extraordinary that so large a substance as the head of a fœtus at the full term of gestation should make its exit through a space ordinarily of no more extent than eighteen lines, or thereabouts; but the extensibility and dilatability of which it is capable, and which is exemplified in every accouchement, render any doubts arising as to the possibility of such occurrences easy of solution. I had doubted the propriety or expediency of making an incision in the perineum for the purpose of facilitating the birth of the child until I met with the case related at the commencement of this article. I now think that when the perineum is threatened with laceration the superiority which a clean incision obviously possesses over a forcible rupture (when such is likely to be inevitable) must be apparent to every one; the sole difficulty (and I confess one not easily to be surmounted) arising from the question, "In what cases is it applicable, and in what cases are we to trust

to nature to effect the dilatation of the parts?" The cases in which it is expedient must be comparatively very rare indeed in the practice of any one individual; yet, if we look at the annals of midwifery, we shall find many recorded cases of extensive laceration, perforation, &c. &c., many of them occurring in the practice of men of the utmost celebrity, amongst whom I may mention Joubert, Coutouly, Merriman, Evrat, Dupuytren, &c. &c., and will see that instances are not wanting where this horrible accident might have been averted by a timely incision.

In a case related by Moschener, the infant was born through the perineum *in consequence of gangrene, induced by the excessive and prolonged pressure of the head.* Might this not have been obviated by an incision? There have been doubts started as to the propriety of the operation in consequence of the possibility of prolapsus succeeding to it; but would it not be equally, if not still more likely to be the after-consequence of laceration? In the case which I have related there has not been the slightest inconvenience experienced as an effect of the incision.

It appears, however, difficult to determine absolutely in what cases it will be required, as (*à priori*) we are not able to say positively whether the perineum will be lacerated or not, until the child is on the point, or rather in the very act of being expelled, when, in most cases, nature is so expeditious that not a moment is left for deliberation.

January 18, 1843.

CONGENITAL INTERMITTENT FEVER.

Dr. Brunzlow, of Brandenburgh, reports the following case:—A woman, thirty-four years of age, was seized, in the second month of pregnancy, with a tertian intermittent fever. This lasted for several weeks, when it was cut short by the use of bark. A return of the ague, however, soon occurred, assuming the quartan type, and persisted till the seventh month of pregnancy; it then seemed to have yielded for a few days, but it re-appeared in the eighth, and continued till a period in the course of the ninth month, when it was finally got rid of. The patient was delivered of a small and feeble child, which four months afterwards was observed to be very restless, fractious, and feverish, every fourth night. Dr. Brunzlow soon satisfied himself that the child was the subject of a quartan intermittent, the three distinct stages of which lasted usually during a whole night. He treated it by frictions over the pit of the stomach and in the armpits with a solution of sulphate of quinine, which medicine he also administered internally to the extent of a grain and a half in the twenty-four hours. Under this treatment the child soon recovered.—*Gazette des Hôpitaux.*

THE PHYSIOLOGY OF THE SPLEEN.

To the Editor of The Lancet.

Sir,—In The Lancet of Dec. 3, 1842, there is a notice of some microscopical observations conducted by Mr. Stevens, of Kennington, to determine the structure of the spleen, and the office this organ performs in the animal economy. As the papers of this gentleman are in the hands of the Royal Society, and their contents are yet undivulged, it is impossible to decide upon the merit of his investigations. Praise or censure, therefore, would be equally futile. Whether, however, your statement of Mr. Stevens' views be correct, respecting the necessity of a capillary circulation interposing between the union of arteries and veins; and whether, the statement being correct, the principle which it announces be true, must be left for consideration when the papers are authenticated by publication. We must not, meanwhile, forget that branches from the hepatic artery pass *directly* to branches of the vena porta. Waiving these objections, I am happy to find that the attempts of Mr. Stevens to elucidate the function of the spleen by a microscopical examination of its structure has produced results so entirely coinciding with inferences I have drawn from pathological data.

My attention was first directed to this subject, some five or six years since, from remarking that a patient, who was extremely lean, yet had an active appetite, complained of sense of *heat*, fulness, and occasionally of obtuse pain over the region of the spleen. There was evidently congestion and enlargement of this organ, and the disease proved most intractable. I have, since that time, often observed an inveterate leanness, generally accompanied with an activity of appetite that might be called morbid, in concurrence with the same deep-seated heat and fulness towards the posterior and lateral part of the left hypochondriac region; thus clearly directing attention to the spleen as the organ at fault. Death arising from this cause alone is, however, so uncommon, that I have never had an opportunity of observing the post-mortem appearances; although in cases of death from other causes, where there has been gradual and extreme emaciation, I have very generally found hypertrophy or extreme congestion of this organ. As a disease, however, operating on the living body, I am confident of its existence.

The reverse condition of deposit of adipose matter in dependence on a small spleen I have been enabled to determine by a post-mortem examination of a dog that died in a state of great obesity. Observations derived from comparative pathology are as good, in this case, as any others.

In April, 1842, a dog, ætat. 3½, of the

bull-terrier breed, short legs, broad chest, and large abdominal organs, and that had, for the previous year or two, become enormously fat and indisposed to exercise, fell into a listless, febrile state, and suffered from violent diarrhœa. In a few days he died.

Post-mortem.—Considerable collection of fat in the adipose tissue underneath the skin; the omentum loaded with fat an inch and a half or two inches deep, and about six inches square, forming a large cake; the mucous membrane of the bowels highly inflamed in various places. All the organs healthy, with the exception of the spleen, which was *very small and dense*, appearing like a kidney, but even firmer and smaller; there were no deposits in its structure, and it did not seem to be otherwise abnormal.

The deposit of fat and inflammation of the bowels in this case doubtless depended on the density and smallness of this organ; and this view will receive confirmation from a detail of the results of an experiment performed by Mayo, and related in his Physiology. This physiologist extirpated a dog's spleen; the animal gradually grew very fat, and when he died, which happened about two years after, his intestines were found to be inflamed and his omentum to be loaded with fat. These symptoms were considered accidental; but they are so exactly similar to those that occurred in the case above narrated, that I cannot avoid considering them dependent on a similar cause; in the one case on the absence of the spleen, in the other on its abnormal state. It is true that the spleen has been often removed, and it is said that in some cases no apparent difference has ensued in the habits or condition of the animal; but this may have arisen from the operators contracting the period of their observations, and not allowing sufficient time for a diseased action to be set up; for, although a sudden effect might not be produced, yet a gradual change in the organic functions might surely be expected to succeed to the extirpation of so large an organ. To enumerate the various symptoms said to have been the effects of the operation would be useless, as they are often contradictory, and often disallowed, and frequently appear to have had their origin rather in an anxious fancy than to have been the observations of cool discrimination.

If we regard the subject also in a physiological point of view, and attend to the changes carried on in the chyle by the presence of the fluid of the lymphatic vessels of the spleen in the lacteals, we cannot have a doubt but that this organ is one of the most important in contributing to the necessary transformations of adipose matter. According to Tiedemann and Gmelin, the chyle is composed of fibrine, serum, and fatty matter. Before the lacteals pass through the glands of the mesentery and receive the lymph from the spleen, they hold in the chyle a *large* quantity of *fatty matter* from the food, and a *small* quantity of *fibrine* and *colouring matter*. When, however, the lacteals have received the lymph from the spleen the conditions of the chyle are reversed: the fibrine and colouring matter increase, and the fatty matter diminishes in quantity. Now it is apparent that the fatty matter undergoes some change on the admission of the lymph from the spleen into the lactiferous vessels.

The lymphatics of the spleen carry a fluid containing a much larger proportionate quantity of fibrine and colouring matter than the roots of the lacteals; consequently, when the mixture of the two fluids takes place, a given quantity of chyle will contain a larger ratio of these components than it previously exhibited. This, therefore, is one reason why the fibrine, &c., should be in excess after the junction of the lymphatics of the spleen with the lacteals. It will further appear, however, that in proportion to the quantity of fibrine will be the disappearance of free fatty matter. The fibrine and fatty matter have a peculiar affinity for each other, and, in the blood, are always found in combination. This affinity seems to be a necessity of the animal economy; for this reason, the serum, although containing fatty matter in union with albumen, never gives out carbonic acid; but the fibrine in which the oily matter and colouring particles reside, as if by especial affinity, can generally, according to the most accurate investigations, be made to develop carbonic acid. It would seem, therefore, that the contact of the fatty matter and the colouring matter, by which the one surrenders oxygen and the other carbon and hydrogen, to form carbonic acid and water, is necessary for the due development of these important chemical processes. This threefold affinity appears to me to be a necessary condition, and involves many important considerations that I have not time to enumerate.

The spleen is deeply engaged in the accomplishment of these changes. When the fibrine contained in its lymphatic vessels is thrown into the lacteals in greater abundance than usual, a larger proportion of oily matter is thereby taken up and carried into the circulation in a state of combination, and there is, consequently, a greater disposition to the formation of carbonic acid, and the oily matter is not deposited. On the contrary, when the fibrine from the spleen is deficient the fatty matter does not enter into combination; it is free in the chyle, gives a milky character to the serum of the blood, and is deposited in the adipose tissue.

The formation of water and carbonic acid out of the oily matter is not, however, the only use of the fluid of the splenic lymphatics in the lacteals. I firmly believe that there is a gradual conversion of oily matter into aoztised principles, and that the fluid of

these vessels materially assists in effecting this change. As this view is almost, if not entirely, new, and would require an elaborate examination to establish its truth, I shall not dilate upon it here, but shall take it into consideration in connection with another subject, on which I am at present engaged.

There is another physiological fact tending to prove these positions: when the ductus choledochus is tied the chyle is clear, and contains only a small apparent quantity of free fatty matter. In consequence of this, it was supposed, by Tiedemann and Gmelin, that the bile was a solvent of fatty matter; but other physiologists have proved this opinion to be false, and the fact remains yet unexplained. In this case, then, from closure of the natural passage of the bile, congestion of the liver must ensue, and, in consequence, congestion of the spleen, whose veins empty themselves into the vena porta. Now the spleen being *congested*, its lymphatics acquire an increased action, and carry an additional quantity of fibrine to the lacteals; and, in accordance with the law which I have unfolded, the chyle must necessarily be clearer than usual, and contain less free oily matter. This congestion of the spleen also explains the pathological condition of leanness, whilst its atrophy, or absence, accounts for the excess of fatty matter in the tissues.

A German physiologist, whose name I cannot at this moment remember, has proved that oily matter combines with albumen (and collaterally with fibrine) by mixing them together; and he has observed that the albumen forms a pellicle around the oil globule; a circumstance that I have also observed. I am, Sir, your most obedient servant,

GEORGE ROSS.

Enfield, Middlesex,
Dec. 6, 1842.

FUNCTION OF THE SPLEEN.

To the Editor of THE LANCET.

SIR,—I pass over, as childish and nonsensical, the allegation by Mr. Stevens, that I read his paper hypercritically or inattentively, and at once address myself to the subject.

In my note of Jan. 15, LANCET, p. 623, I noticed one grand deficiency in Mr. Stevens' paper, the absence of any analysis, either sensual or chemical, demonstrating that the splenic vein contained arterial blood. Here is Mr. Stevens' reply,—"I took in several phials separate specimens of arterial and venous blood from a dog, as well as some from its splenic vein. There was not one surgeon to whom these were shown who did not judge the latter to be arterial blood." Now, whatever value might have attached to this mere visual examination (for it does not merit the name even of *sensual* analysis) Mr. Stevens at once negatives by his repeated affirmations that it is not *entirely* arterial blood, thus making the above *judgment* appear truly ridiculous. Mr. Stevens says, "The blood of the splenic vein is chiefly and essentially arterial, and would be wholly so but for the small portion of the residual blood of the splenic nutrition (by the vasa vasorum)." To this proposition I have objected on sensual analysis, and again here object that the blood of the splenic vein is not chiefly and essentially arterial,—that it is greatly deficient in fibrine and the other constituents of arterial blood,—that, in short, it is the colouring matter of the blood in its primitive condition, first rendered susceptible of aeration in the liver.

Now, it will be seen, that my objection *does* militate in a *great degree* against what Mr. Stevens has advanced, the analysis of the contents of the splenic vein being the "experimentum crucis" by which Mr. Stevens' theory must stand or fall. And should no analysis of the above nature be offered to THE LANCET pending this discussion, I will use my endeavours to procure it the first opportunity from some philosophical chemist, competent to the task, and in that case will frankly forward the result to THE LANCET.

With respect to Mr. Jackson's theory, that the spleen is an "assistant circulatory organ," I object that it has yet to be demonstrated that either the spleen, properly speaking, or its capsule, or the portal vein, possesses any vital contractility whatever; "and," says Mr. Jackson, "he who denies their vital contractility also denies their propulsive power" (LANCET, p, 89). I am Sir, your obedient servant,

FRANCIS EAGLE.

Jan. 30, 1843.

To the Editor of THE LANCET.

Sir,—I hope you will believe me when I assure you that I have no desire to force my opinions upon any of your readers. I have merely ventured to give a short and, I fear, an imperfect detail of certain microscopic and anatomical experiments which seem to have discovered (to me most satisfactorily) the function of the spleen and the placenta; and the professional congratulations which I have received have led me to feel the more confident in this opinion. Perhaps I am wrong, and am labouring under a monomania. According to Mr. John Jackson's *dictum* (your correspondent of last week) I am decidedly so, in every essential particular but one; but yet none of Mr. John Jackson's reasons for thus extinguishing me are exactly satisfactory to my mind.

Mr. John Jackson says that Mr. Stevens—

First. "Is wrong in stating that 'the spleen comprises a mere congeries of capillary vessels,' for the spleen, we all know, is

remarkable for its distensibility, which *capillaries* are not."

I was ignorant enough to suppose that capillary vessels often became distended and visible to the naked eye during congestion.

Secondly. "He is wrong, too, in stating that its capsule is a prolongation from the aponeurotic tissue of the diaphragm."

The fibrous tunic is very thick at the root of the spleen, passing directly into the aponeurotic tissue of the diaphragm, as a strong supporting ligament. This is plain in the sheep and in the ass.

Thirdly. That "he is equally wrong in supposing the placenta to be a double and deciduous body of capillaries between the uterine arteries and the umbilical vein, and the umbilical arteries and the uterine veins, and in believing that the blood not only passes from the mother through the placenta to the fœtus, but also back from the fœtus through the placenta to the mother; for if blood did return from the fœtus to the mother, it is evident the terminal branches of the umbilical arteries in the placenta would not be continuous with the radicles of the umbilical vein."

Quite evident; and here I venture to say that it is not in the power of Mr. Jackson to bring me an injected placenta showing any proper communication between the umbilical arteries and what he calls the radicles of the umbilical vein.

Fourthly. "He is, moreover, wrong in the version which he has given of my theory as to the use of the spleen; for he appears to imagine that I believe the spleen, *per se*, propels the portal blood through the liver; whereas I do not regard the spleen as an isolated organ, but as the roots of the spleno-hepatic vein, and that it is that vein which propels the blood through the liver."

I apologise for having done Mr. Jackson this injustice; but (excuse my ignorance) do the hæmorrhoidal veins form any part of the roots of this spleno-hepatic vein? And if so (as I suppose), do they belong to the spleen?

Fifthly. "But that in which Mr. Stevens is most wrong is in supposing the large size of the splenic artery is for the purpose of supplying a large quantity of oxygenised blood to the portal blood, or that the spleen is for the purpose of diverting arterial blood into the portal vein; for if it were so, the size of the splenic artery and the spleen would bear pretty nearly the same proportion to the liver in all animals, and would not be more than twice as large relatively to the liver in man as they are in the quadruped."

What quadrupeds has Mr. Jackson examined? I know of no such law applying to the whole class; and it varies greatly in fat and in lean men. But has Mr. J. taken any pains to examine the splenic blood as regards oxygen and carbonic acid?

Sixthly. "The reason the splenic artery is a large artery is because it is necessarily proportionate to the quantity of *venous radicles* of which the spleen essentially consists."

I was under the impression that the notion of *venous radicles* was exploded. The microscope has failed to define to me where the arteries cease, and where the *venous radicles* commence. They have always seemed to me to merge into one another, and to be directly joined.

Seventhly. "Mr. Stevens is right in remarking that no change is effected on the blood in its progress through the spleen, but in every other essential particular he is wrong. The last number of THE LANCET contains a note from Mr. Eagle, in which he observes that the blood in the splenic vein is neither venous nor arterial. Mr. Eagle is perfectly right there. *The portal blood is diluted blood, for every ounce of fluid received by the stomach passes into the splenic vein.*"

Pray, by what set of vessels does all the fluid received by the stomach obtain admission into the splenic vein?

Since all these points were fairly stated in my original paper, and since no experiments have been brought forward in opposition to mine, what can induce these gentlemen to go off at "half-cock," not having first duly considered what I have done and described?

I remain, Sir, yours most respectfully,

ROBERT STEVENS.

Kennington, Jan. 28, 1843.

DR. AYRES' REMARKS ON LIEBIG'S VIEWS.

To the Editor of THE LANCET.

SIR,—The note of your correspondent, "D.," at p. 440, (or, as I shall style him, *Debilis*, from the character of his remarks), would require no comment, were it not that some among your readers may have misunderstood both my reasoning and objects (p. 342). It is amusing to find the *argumentum ad verecundiam* of the old logicians brought forward at the present day, when everything new is subjected to the most severe ordeal of discussion. Your correspondent appears to belong to the middle ages, and has constituted Liebig his Aristotle, to speak against whose opinions is heresy. His argument amounts to this: "Because Liebig's opinions have been subjected to the German physiologists and public and have been approved by them, therefore they are absolutely true!"

Your correspondent must certainly have misunderstood me in my argument as to the existence of both locomotion and sensible motion in plants and animals; for he seems to infer that I had taken up your space in the vain attempt to find an absolute distinction between the two kingdoms at their point of union. The fundamental axioms of Liebig on the distinction of motion in animals from

that of plants, is made of considerable importance in that part of his work which treats of "the phenomena of motion in the animal organism," and for that reason I attacked it; it being always of more importance to examine principles than details. Besides, I have not confined myself, in the course of the argument, to the lower tribes of plants, but have shown that *sensible motion, arising from internal causes*, occurs in many of the most highly organised. What will be said to the motion of the leaflets of hedysarum gyrans?

I have deferred the present communication rather longer than I otherwise should in order that I might have an opportunity of consulting Burdach. I have done so, and am pleased to inform your censorious correspondent that Burdach's text fully accords with my views. As I am not sufficiently acquainted with the German language to read it with facility, and as Jourdan's translation was the only accessible edition of Burdach, I hope that "D." will be satisfied with the passage to which Liebig refers in French. I shall leave it to you, Mr. Editor, either to print it as I send it, or to give your own translation, as "D." seems to have so bad an opinion of me that I dare not trust myself to make it. Here is the passage:—

"Dans les vivisections on voit de temps en temps, environ deux fois par minute, chez les chiens, d'après Magendie, une goutte de bile couler du canal biliare dans l'intestin. Aussi Graaf a-t-il recueilli six grains de ce liquide en huit heures (minutes?) sur un chien, et Keil deux gros en une heure sur un chien de grande taille, faits d'après lesquels Haller pense qu'on peut admettre que la sécrétion biliare s'élève à environ vingt-quatre onces en vingt-quatre heures, chez l'homme, *evacuation évidemment trop forte pour ce qui concerne l'etat normal.* Comme le chyme est acide, et que cet acide est neutralisé par la bile, Schultz calcule la quantité sécrétée de la bile d'après celle du chyme produit, et d'après celle de la bile nécessaire, en vertu de ces expériences pour neutraliser l'acide de ce dernier. En procédant de cette manière, on trouve qu'un gros chien sécréterait par jour trente-six onces, en un bœuf trente-sept livres et demie de bile. Or, si le bœuf avait autant de salive que le cheval, comme le pense Schultz, c'est-à-dire dix livres, il resulterait que ces deux sécrétions seules égaleraient, et vingt-quatre heures, la masse totale du sang."*—(Burdach "Traité de Physiologie," par Jourdan, tome vii. p. 429.

I will not commit more "ink-shed" than is absolutely necessary for my defence from the observations of "D.," and therefore leave your readers to compare my statements with the actual passage from Burdach. As a matter of curiosity I shall append an extract from Schultz. He says:—

"Maxime necessarium autem esse bilis usum in digestione præterea ex uberrima ejus copia elucet. Et primum quidem ex experimentis supra allatis de saturatione definita chymi portionis cum bile apparet, post ventriculi digestionem in herbivorum duodeno duplicem bilis portionem ad chymum saturandum requiri, in carnivoris propter majorem ascentiam triplicem fere. Insuper vero in intestino cœco adhuc dimidia bilis pars ad saturandum chymum necessaria est. Cum vero in sana digestione tam intestinorum tenuium quam crassorum ingesta postremum vel perfecte neutralia vel alkalina a superante bile inveniantur, facile e certa chymi quantitate in aliquo animali intra 24 horas facta computari potest quæ bilis quantitas ad illam saturandam desideretur. Major canis, qui tantum pastus per diem sumpsit, ut inde 12 unciæ chymi orientur ad hunc bene digerendum 36 uncias bilis egebit. Bos taurus bis quotidie rumen humido pastu impleus per omnia quotidie 15 libras chymi perficiet, quæ exposcunt ad saturationem in duodeno 30 libras atque postea in cœco 7½ libras bilis, sic ut per omnia circiter 37½ libras bilis intra 24 horas a bove gigni necesse sit. Hæc bilis quantitas salivæ quantitatem, intra 24 horas ab æquo exhibitam, longe multumque superat, majoremque bilis hepatisque gravitatem esse demonstrat."†

* In vivisections we see, now and then, about twice a minute, among dogs, according to Magendie, a drop of bile trickle from the biliary duct into the intestines. Graaf, also, has collected six grains of this liquid in eight hours (minutes?) from a dog, and Keil two drachms in one hour, from a dog of large size; according to which Haller thinks that we may admit the biliary secretion to rise to about twenty-four ounces in twenty-four hours in a man, an evacuation which is plainly too great as regards the normal condition. As the chyme is an acid, and as that acid is neutralised by the bile, Schultz calculates the quantity of bile secreted according to that of the chyme produced, and according to that of the bile necessary, by reason of these experiments, to neutralise the acid of this last (i. e., the chyme). By proceeding in this manner we find that a large dog would secrete daily thirty-six ounces, and an ox thirty-seven pounds and a half of bile. Now, if the ox had as much saliva as the dog, as Schultz thinks, that is to say, ten pounds, it would follow that these two secretions alone would, in twenty-four hours, equal the entire mass of the blood.

† Besides, it is manifest, from its very plentiful abundance, that the use of bile, in digestion, is especially necessary. And, first of all, it appears, indeed, from the experiments alleged above, with regard to the definite sufficiency of the proportion of chyme with bile, that a double proportion of

I regret that I had not time to examine the grounds of Schultz's statement concerning the quantity of the chyme furnished by different kinds of food, upon which his estimate of the quantity of bile is evidently founded, but must leave this investigation to those who are in the neighbourhood of a large library and have leisure to read the whole of the essay. Your obedient servant,

PHILIP B. AYRES, M.D.

Thame, Jan. 4, 1843.

PROTRACTED CONSTIPATION.

To the Editor of THE LANCET.

SIR,—I lately attended Mrs. C. during a violent attack of colica ileus (iliac passion), and as my patient recovered after the bowels had been obstinately constipated for seventeen days, and incessant vomiting of fæces had continued for fourteen days, you may consider the case sufficiently interesting for notice in your widely-circulated Journal; but as your space is too valuable, and I have little time for lengthened details, I shall content myself with briefly observing that I mainly attribute my success to the continued and persevering administration of large doses of calomel, conjoined, in the early stage of the disease, with one full bleeding from the arm, and the application of leeches and blisters to the abdomen. Of course, I also employed those remedies most worthy of confidence, such as saline effervescing aperient draughts, with sedative and purgative enemas, and, to relax spasm, injections of hyoscyamus, soap and opium suppositories, and though last not least, glysters of tobacco infusion, at intervals of a few hours, until giddiness was produced; but these I regarded as auxiliaries, and depending chiefly upon calomel persisted in its use until the obstruction in the intestinal canal was overcome. Some years ago I also had under my care a man with ileus, who is now in my service, which resisted all my endeavours to relieve him for ten or eleven days, and this case I likewise at last subdued by large doses of calomel. Although I have reason to believe that such instances of recovery as those to which I now particularly desire to call the attention of your readers, are extremely rare, yet when they happen they should teach us never to despair, nor relax in our efforts until we conquer the disease, or death closes the scene, a result which, under the best and most skilful treatment, will, alas! too frequently occur. I remain, Sir, your obedient servant,

GEORGE YEATES HUNTER, M.R.C.S.

Margate, Jan. 27, 1843.

bile is required to saturate the chyme in the duodenum of herbivorous animals, after the digestion of the stomach; and that a threefold proportion is generally required in carnivorous animals, *propter majorem ascentiam triplicem fere*. Moreover, also, in the cœcum, half the proportion of bile is necessary to saturate the chyme. But, inasmuch as in sound digestion, as well of the lesser intestines as of the greater, either perfectly neutral salts or alkalies are found at last to be carried into them from the overplus of bile, it may easily be calculated, from a determinate quantity of chyme made in any animal within twenty-four hours, what quantity of bile is required to saturate it. A large dog, who takes so much food during the day that from it twelve ounces of chyme will be made, will need thirty-six ounces of bile to digest that food. A bull filling its stomach (*rumen*) twice every day with moist food, will make altogether, daily, fifteen pounds of chyme, which require thirty pounds of bile in the duodenum, and, afterwards, seven pounds and a half in the cœcum, to saturate that chyme; so that altogether thirty-seven pounds and a half of bile are required to be secreted by the bull within twenty-four hours. This quantity of bile exceeds, beyond all comparison, the quantity of saliva produced, *ab æquo*, within twenty-four hours, and evidently proves the weight of the bile and liver to be greater.

ROSEMARY DURING PREGNANCY.

THE injurious effects of this herb, taken internally, are illustrated in the following case, reported by Dr. Thierfelder, of Meissen. A female, eighteen years of age, of a delicate constitution, and in the fifth month of pregnancy, was treated by an old woman as for ascites, and by her advice took a strong decoction of rosemary, with the view, as was supposed, to excite the action of the kidneys and promote absorption of the effused fluid. On the day after that in which the decoction was first taken, headach, vertigo, and sleeplessness, manifested themselves, and progressed in intensity till the fifth day, when a difficulty of respiration and cold chills induced or compelled the patient to keep her bed. At this time, also, labour came on, accompanied with considerable flooding, which terminated in the birth of a still-born male fœtus. Syncope, spasms of the chest, and coldness of the extremities, continued to prevail, and in addition, the pulse became small, frequent, and irregular, evincing extreme jactation of the entire system. Dr. Thierfelder, who was then called in, attacked and checked some of the symptoms by valerian, camphor, acetic ether, and other powerful antispasmodics, but the great prostration that had been occasioned to the system necessitated a long-continued course of tonic remedies, with a diet of the most nourishing kind.—*Summarium*, 1842, No. 30.

THE LANCET.

London, Saturday, February 4, 1843.

Last week we expressed a belief, in this division of our Journal, that the information of the medical profession, generally, on matters of natural science, was very little greater than that of the people at large. This is an extremely humiliating fact. If medical practitioners—the professors of an art which touches the circle of the sciences at almost every point—are not much farther advanced in the knowledge of natural causes and effects than the mass of the people, whose occupations, for the most part, rather lead them away from the study of nature than aid than in its pursuit, that argues a want of mental energy on the part of those practitioners, which is much to be lamented; for they ought to be so decidedly in advance of the general community in natural knowledge, as to become capable of acting as the interpreters of nature to their fellow-men in all things relating to their physical well-being. But before throwing unqualified blame on any set of men on account of their intellectual condition, an inquiry should be made into the causes which have led to that condition. Now, we apprehend that there are two principal causes of the state of things which we are regretting. First, a vile system of medical legislation, which has demoralised the profession, and deprived its members of that self-respect and elevation of mind without which no exalted end is to be attained. Secondly, a false relation of the profession to society, which is, in a great measure, peculiar to this country, and entails on the medical practitioner a particular mode of living, without which he has little chance of obtaining an extensive practice. In consequence of this, his time and his means are occupied with keeping up a certain establishment and expenditure, which are not at all essential to his real position as a medical practitioner, but are a sacrifice to the prejudices of society. He retains, thus, neither time nor means for acquiring that scientific improvement which would render him additionally respectable in the eyes of sensible men. He becomes one of the numerous slaves of ostentation, the prevailing vice and curse of this country, which extends its baneful influence through every order of society, and, as if by an unhappy fatality, assumes every form but that of *nationality*, the only form in which, if the vice do not pass into a virtue, it at least puts on the semblance of one, and conduces to magnificent results.

But not to digress. The deficiency of medical practitioners in scientific knowledge is to be deplored, not only as involving the absence of an immense amount of useful information, but because it implies a want of that expansion of mind, that philosophic acumen, and that aptitude for reasoning, which are necessary to make a medical practitioner fit for some of his highest functions. There are many instances in which the medical practitioner is called on to assume the character of a popular teacher, and to enlighten his fellow men on points which can only be elucidated by one who is endowed both with a philosophic frame of mind and with medical experience. One of the most obvious and important points on which the public mind stands in need of guidance from the medical profession. is that of the pretensions of quacks —the merits of their systems, and the analysis of the lying wonders with which they are wont to mystify the public. To this point especially we now wish to direct attention.

It should be the peculiar province of the medical profession to guard society against these impositions, not by a stolid rejection of alleged facts merely because they are new, or extraordinary, but by an acute examination and philosophical analysis, whereby any truth which is mixed up with the mass of error and deception may be fairly eliminated, and those obliquities of reasoning detected which would divest the most valuable truths of their utility by giving them a false application.

But not only would medical men find that

the study of general philosophy and the principles of right reasoning would enable them to exempt the public mind from the dominion of quackery; they would frequently find, also, that their own science derived important light from a just analysis of the very delusions which they were called on to dispel. In fact, it very seldom happens that a system takes extensive hold of the public mind, without containing something true and useful, though it very often happens that this true and useful part is in diametrical opposition to the ostensible claims of the system to public consideration. Let us see how far these observations will apply to some of the prevailing delusions of the day.

Of *mesmerism* we are too sick to descant upon it at any length, and there are but few members of the profession so utterly devoid of reasoning power as not to know that the mesmeric phenomena, in so far as they are true, form merely a heterogeneous mass of materials for the study of psychical medicine, and are wonderful only to those who are unacquainted with the aspects of disease. When we continually see patients labouring under hysteria, and analogous forms of nervous disease, falling suddenly into various states of stupor, trance, and convulsion, without any assignable cause, why should we wonder at similar states being induced by so slight a cause as the pawing of a mesmeriser? And, knowing, as we do know, the remarkable power of habit in facilitating the repetition of nervous actions which have once taken place, why should we wonder that an effect which, a few weeks ago, it took half an hour's handy-work to produce, is now occasioned by two minutes' application of the same graceful process? In these things there is nothing either new or wonderful. With regard to those exalted mesmeric states which would, indeed, be wonderful *if true*, all that we have to say is that the professors of the "science" have already submitted some examples of those states to our inspection, and subjected their reality to an effectual test, thus enabling us to place the public in a state of broader "clairvoyance" than was agreeable to the parties first concerned in the matter, and to diffuse a light over the whole subject of "exalted" mesmerism which threw even its "universal lucidity" into the shade.

Next, glance at *homœopathy*. This is at present the most widely spread of medical delusions; and that because it envelops in its mysticfications two very important general truths in medicine, and one emphatic precept, chiefly applicable to British practice. First, it is perfectly clear to common sense that to give a man the six-millionth of a grain of any substance, however active, is practically equivalent to giving him nothing. Homœopathy, then, justly interpreted, becomes synonymous with *expectant medicine*.

But, secondly, good practitioners have always been aware that there is a considerable proportion of cases in which drugs are injurious, and in which the proper treatment consists in so regulating the diet and habits of the patient as to give nature time and fair play to overcome the disease. Is it wonderful, then, that some cases which have been wrongly treated by the regular faculty—inasmuch as the patients have been surfeited with drugs where no drugs were required—prosper better in the hands of the homœopathists, who do that which is tantamount to doing *nothing*? "Hold!" say the homœopathists; "This is very fine talking; but what if we "can show you cases in which our infinitesi- "mal doses do actually produce perfectly "manifest and powerful effects, such as "violent vomiting and purging?" We answer that in a few cases only will such effects be produced, and that those are analogous to instances in which the sensorium exerts so strong an influence on the bodily organs, that a little coloured water, given with the assurance that it is a powerful emetic, will produce vomiting; and analogous, also, to those other instances in which persons, labouring under a like sensorial influence, become sea-sick the moment that they go on board a ship, although she be lying perfectly motionless in smooth water. The fact is, that in such cases the effects

produced have no immediate relation to the infinitesimal globule, nor the coloured water, nor the motionless ship; these are all but signs of a thing signified, and it is to a predominant impression in the mind, and the powerful, though unexplained, action of the brain on a distant organ, that the curious phenomena in question are to be attributed. Homœopathy, then, illustrates a very interesting fact in psychical medicine. But we have said that homœopathy affords also a very important precept which is especially applicable to the practice of medicine in our own country. It is this:—The expectant principle, which is the essence of homœopathy, contrasts very powerfully with the drugging principle which, unhappily, is too often the essence of British practice, and the contrast is by no means to the advantage of the latter.

It is this same drugging system which forms the foil on which infinitesimal medicine glitters. Large fortunes are made by homœopathy in London, but not so in Paris, because, in the latter city, it lacks our drugging system with which to place itself in advantageous contrast. Drs. Quin and Curie may imagine that their best friends are among dowager countesses and listless ladies of fashion, but we can assure them that there is another set of females to whom they are infinitely more indebted, namely, the old ladies at Rhubarb Hall, who have done all that in them lay to convert the medical practitioners of this realm into pill-mongers. If the above-named gentlemen do not bequeath at least one half of their fortunes to the tabbies at Blackfriars, they will be excessively ungrateful.

And now a word about *hydropathy*. This certainly cannot be accused of being expectant medicine in disguise; we should rather say that it was hygiene run mad. It will not have so long or so successful a course as homœopathy, because, although it may effect some very striking cures, it will kill so many people outright that it will soon be brought to an end by coroners' verdicts. The philosophy of the "water cure" is extremely simple. First, there is a number of cases in which the internal and external use of cold water have long been recognised as excellent remedies. In these cases, there is no reason why the remedy should lose its efficacy, because administered under the new name of "hydropathy," though, possibly, some cautious persons might be apt to refer the selection of the cases in which the remedy was properly applicable, to well-informed practitioners, rather than to ignorant empirics; secondly, there are, doubtless, cases in which cold water has not hitherto been considered specific, in which, nevertheless, the "water cure" might work wonders through the medium of a rather hazardous process. Imagine a fat citizen, who has not walked ten miles on a stretch for as many years; the entire surface of whose body has never been simultaneously visited by cold water since his childhood, and who would regard the prospect of such visitation with feelings little short of hydrophobia; whose stomach has been overworked with too much alimentation, and whose brain has been overwrought with too much business; and who is, from the conjoint operation of these causes, debilitated, dyspeptic, and hypochondriacal. Send this individual to Silesia, and place him under the discipline of Priessnitz. Let him pass whole days exposed to the free mountain air; let him get an appetite for plain food by hard exercise; let his mind be set free from the cares of business, and, not to forget the mighty agent, let him be frequently and thoroughly drenched, inside and out, with cold water. The result will depend on the degree of vital reaction still remaining in a constitution impaired by unnatural habits of living. The patient has some chance of dying of internal inflammation; but, on the whole, perhaps, the most probable result will be a wonderful renovation of his entire man; and the only objection to hydropathy in his case would be that by a moderate and rational use of the same agents under their ordinary names, his health might have been as perfectly restored without putting his life into the smallest jeopardy.

But let not the feeble imitators of Priessnitz in this country imagine that they will be able to produce results like those of the of the original and powerful minded, though ignorant, German peasant. In Silesia, hydropathy is a grand empirical water-spout, which will swamp all before it, until the untoward demise of some great man, or some whimsical change in public feeling, dissolves the spell of its existence, leaving the world to wonder that it ever had any; but hydropathy in England is a very poor affair, a mere spurt from the perturbed fountain of common-place quackery.

There is something, however, to be learned from *hydropathy*, if it be only a knowledge of the right use of cold water. The restoration of health which frequently results from it should teach us the importance of those natural stimuli, the operation of which is too much excluded by artificial habits. When a man is ill, let him not resort to a water-doctor to be cured, for he will run nearly an equal chance of being killed; but while he is well, let him use those means for the preservation of health which nature has supplied in the elements around him, and he will probably find that the judicious use of water, air, food, and exercise, will keep him out of the hands of all doctors whether terrestrial or aquatic. In fine, the only useful view to take of hydropathy is a hygienic one, which would regard it as a powerful, though exaggerated and distorted illustration of the doctrine of vital relations.

We have presented the reader with a very rapid outline only of what may be called the philosophy of quackery; we hope, nevertheless, we have said sufficient to convince many that medical practitioners are called to occupy a higher position in society than they actually do, but that in order to fit themselves for such advancement they must become natural and general philosophers. We trust, also, it has been sufficiently demonstrated that the prevalence of quackery is in a great measure owing to the deficiencies of the profession, and that the great means of putting it down will be found in the ability on the part of medical men, to strip it of its illusions, to reduce it to its true principles, and to take the little kernel out of each form of quackery as it arises, should any happen to be contained in the shell.

An Exposition of the Pathology and Treatment of Tubercular Phthisis. By Samuel Flood, M.R.C.S.L., &c. 12mo. Pp. 82. London: Simpkin and Co.

This subject, hacknied by some commentator or other in every street, has, we think, found a faithful interpreter in Mr. Flood. If brevity be the soul of wit, as it assuredly is, this little volume may boast of one quality, at least, of a rare jewel, for it affords to us, in the brief space of eighty duodecimo pages, or thereabouts, a clearer insight into the pathological cause of phthisis, than is presented in nearly all the elaborate works on that disease by which it has been preceded. The *primum mobile*, the real source of this wasting scourge of humanity, has been strangely overlooked or mistaken, both by authors and practitioners, who, in the words of the book before us, instead of "endeavouring to check the torrent itself, occupy time in intercepting the straws which only serve to show how the current is running." The object of the medical attendant has but too often been to quiet the cough and stop the purging or the night-sweats, &c.; and he has only given temporary relief to the patient, instead of striking at the root of the evil, and prescribing for the *marasmus* which is the most strongly-marked and formidable symptom of the malady. Phthisis Mr. Flood shows to be essentially and fundamentally a defect in the function of nutrition, from disease of the mesenteric glands and consequent obstruction in the lacteal vessels and the progress of the fluid they carry. And although he neglects neither a careful attention to the state of the lungs, nor the necessity of keeping up the strength of the patient, yet to these indications he adds a third, that of restoring, if possible, the healthy state of the mesenteric glands,—an indication possessing, as he considers, an importance which is decidedly paramount to that of the other two. Mr. Flood asserts that consumption, so far from being irremediable by medicine, is, if attacked sufficiently early, perfectly *capable of cure*, though not by the medicinal agents which

have hitherto been those mostly employed for combating it. The means by which he proposes to effect a cure are, however, within the reach of every medical practitioner, *iodine* with cantharides being the sheet-anchor of the treatment recommended.

"In every disease (says Mr. Flood) having a scrofulous origin *iodine* stands pre-eminent, but in the treatment of phthisis it is quite unsuccessful except in combination, and the medicine with which I combine it is *cantharides*. The mixture of iodine and cantharides seems sometimes to act as a specific. I have seen disease yield to it at once, which has obstinately resisted the iodine alone for months. * * The proportions I have generally used are, for an adult, iodine half a grain, iodide of potassium a grain, and tincture of cantharides twenty minims, in some appropriate menstruum. * * In reference to its administration, I prefer giving it immediately after every meal, as it then becomes intimately mixed with the food, and is presented uniformly with it to the mouths of the absorbent vessels. Its beneficial action is very much promoted if it be omitted for a day occasionally, and if on such days a brisk purgative of calomel and rhubarb be substituted."—Pp. 40, 41.

In the two pages preceding, Mr. Flood points out that when the mesenteric glands are to be felt prominent and enlarged on exploration of the abdomen,—

"And especially if the examination discovers any tendency or any signs of uneasiness on pressure, a few leeches may be applied with marked advantage, and these may be repeated; or from four to six ounces of blood may be withdrawn by cupping. This loss of blood should be followed immediately by a blister, which, however, should not be permitted to heal up in the usual way, but the cuticle being stripped off it should be dressed with iodine ointment; and such blisters may be repeated over and over again for many weeks, taking care to have them only of a moderate size, and altering the position of each successively to a different part of the abdomen. * * When we can no longer continue the blisters the good effects of the medicine may still be kept up by laying over the whole of the abdomen an iodine plaster, spread on leather, in the proportion of a drachm of pure iodine to an ounce of emplastrum resinæ."

In extracting thus largely, if we have stepped somewhat out of the *legitimate* province of the reviewer, the acuteness of the views which lead to the above practice, coupled with the lamentable frequency of phthisis, form an ample apology. Mr. Flood suggests the employment of transfusion, not only in extreme cases, to enable the worn-out system to grapple a little longer with the disease, but in the course of the treatment, before the patient's powers are exhausted. And in the course of his work he cites several examples which ought to warn modern surgeons against hastily amputating diseased limbs in subjects of a scrofulous habit, the disease in the extremities being very frequently a salutary metastasis of the morbid tendency, which would otherwise develop itself in the vital organs. The author considers the scrofulous tendency which disposes to phthisis, glandular enlargements, &c., as dependent on a "diseased condition of the blood," a supposition which must, in the present state of our knowledge, be regarded in the light of a pure hypothesis. But there is little else of an hypothetical character in the work, which, so far as it goes, we consider to be among the best practical treatises on this particular subject which have yet appeared, and we have in so saying the writings of preceding authors, from Laennec downwards, in memory.

MEDICAL SOCIETY OF LONDON.

Jan. 9, 1843. Mr. PILCHER, President.

TUMOUR OBSTRUCTING DELIVERY.—PERFORMANCE OF THE CÆSARIAN SECTION.

Mr. T. B. HOOPER, of Reading, communicated the following case:—Mary Davis, ætat. 23, of short stature and robust habit, an inmate of St. Lawrence's Poor-house, Reading, was taken in labour early on the 14th October, 1842 (Friday), and was attended by the midwife in the usual way until the 16th (Sunday), when I was requested to see her. On my arrival, at eleven, a.m., I was informed by the midwife that "she could not find the womb," that the pains had been very strong and frequent all the morning, but that no progress was made. On examination per vaginam a firm body could be felt by the finger, but no os uteri. The hand was then introduced into the vagina with some difficulty, in consequence of the size of the tumour, which was apparently seated on, and adherent or firmly bound down to, the sacrum, and which, rising above "the brim," filled up about four-fifths of "the inlet." The os uteri was now discovered behind the pubis, parallel with the brim, soft, thin, and pretty fully dilated, the membranes entire, and the vertex presenting. The pains continued, and were rather severe. Deeming natural delivery impossible, I requested the opinion of Mr. Maurice, my partner, who, with Mr. Mott and myself, again visited the patient at two, p.m. We

found that the pains had rather abated; the pulse about 100, soft and compressible; the skin warm and moist; the tongue tolerably clean and moist. She complained of pain across the abdomen. She was again examined by Mr. Maurice, both by the rectum and vagina, the tumour being equally felt in either situation. We now ruptured the membranes, and endeavoured to pass the hand within the uterus, both to ascertain how high the tumour reached above the brim, and whether any further room could be gained by pressure, but found that it yielded only so much as to allow the fingers to pass between it and the pubis. It was now determined to leave her for a short time to see whether the continued action of the uterus would cause any displacement of the tumour. At half past six, p.m., we again visited her, Mr. Pilcher being also present, having expressed a desire to see the case. We now found that although the pains had been strong, but little room was gained. The tumour was not so easily reached by the finger, having apparently become slightly flattened and pressed backwards. The scalp over the vertex was much tumefied, and occupied the space between the tumour and the pubis, which did not even now exceed one inch and three-quarters. As no urgent symptoms were present, after puncturing the tumour with a long trocar and canula, to satisfy ourselves that no quantity of fluid was contained in it, and as instrumental interference was impracticable, we again left her, hoping that the head would descend still further.

Nine, p.m. No improvement; the pains had ceased, and she had slept a little. The head had receded, and could be distinctly felt through the parietes of the abdomen below the umbilicus. As she felt inclined to sleep, and as delivery by instruments was even more impossible than before, the space being now more contracted, we left her with the hope that she would be refreshed, and that the pains would return.

17, Monday. Five, a.m. Was sent for by the midwife, who stated that the patient had felt rather faint, and had had no return of the pains; the breathing was somewhat hurried; the pulse small, 120. A little brandy and gruel was administered, but immediately rejected by the stomach. The pulse, however, improved in volume, and she appeared somewhat relieved. Mr. Maurice and my colleague, Mr. Vines, of the St. Mary's district, were now summoned to a consultation, and after duly considering the case it was determined to have recourse to the Cæsarian section, as offering the only chance of saving her life. To this she assented readily, and Mr. Maurice kindly undertook the performance of the operation.

Eight, a.m. The patient being placed on her back, an incision was made from about an inch above the pubis to the umbilicus, exposing to view the linea alba. From the extreme tenuity of this part, the next incision, though carefully made, entered the uterine as well as the abdominal cavity, and which, the finger serving as a director, were then opened simultaneously, thereby much expediting the operation. A gush of venous blood followed the incision. The child was quickly extracted, and the placenta was immediately afterwards thrown off by the contraction of the uterus, which was firm and complete, and entirely arrested any further hæmorrhage. The child had evidently been dead some hours, probably ever since the cessation of pain. The edges of the wound were now closed by means of three sutures and adhesive plaster, supported by a broad towel pinned round the abdomen. Slight faintness was produced, which a small quantity of brandy relieved. She afterwards said that the pain of the operation was not very great, and that she was then more comfortable than she had been for two days. The most distressing symptom which now prevailed was sickness, her stomach rejecting even water, and this continued till within a short time of her death; excepting this, for some time no particularly unfavourable symptoms were observable, the pulse continuing tolerably full and soft, but towards the evening its strength was sensibly diminished, and signs of exhaustion began to manifest themselves.

18, Tuesday. Half-past four, a.m. Was summoned in haste; a considerable quantity of bloody serum had issued from the wound. Some tenseness of the abdomen had been previously noticed, which was now much relieved. The bandage and compress were changed, but the effusion still continued.

Two, p.m. Warm-water dressings were applied over the whole abdomen, and covered with oiled silk; the powers of life were much diminished, and but little hope remained.

From this time she gradually sank, and died at a quarter-past eleven, p.m., about forty hours after the operation.

Inspection, Sixteen Hours after Death.

The body presented the appearance of health; the incisions in the parietes of the abdomen and in the uterus showed no signs of union. The cavity of the abdomen was filled with bloody serum flowing on pressure through the external wound.

Recent peritonitis was general over the whole of the abdominal viscera. There was an abscess beneath the peritoneum, covering the lower ribs of the right side, which was thickened, and adhered by bands of some duration to that of the liver. The uterus was contracted, and the wound gaping, and it now appeared as if made in a slightly oblique direction from opposite the Fallopian tube of the left to about an inch below that of the right side. On the upper edge the

muscular fibres were exposed, the peritoneum appearing retracted to the width of half an inch; the lower one consisted of little besides its peritoneal covering.

On drawing down the uterus the tumour was brought into view, firmly attached to the sacrum by a broad base, passing directly over the conjugate diameter of the pelvis, nearly filling it and resting on the pubis. It was detached from the sacrum with some difficulty, and a bony union was found to exist between it and the first sacral bone, of the size of a fourpenny-piece. The rectum was pushed a little downwards, and to the left side, but was quite unconnected with the tumour, which was apparently of a fibrous character (it was not cut into), with cartilaginous or bony deposit interspersed throughout its anterior part, which is very firm and hard. The posterior contains a small cyst, having had in it two or three drachms of fluid and a few flocculi of unorganised albumino-fibrine; its circumference, when first removed was fifteen inches and three-quarters. The pelvis was well formed, and of full size for a woman of her stature. Thoracic viscera healthy.

Description of the Tumour.

The principal attachment was (by a base about three inches in diameter, partly osseous, partly fibrous) to the sacrum and last lumbar vertebra, protruding against the vagina and cervix uteri, covered anteriorly by peritoneum, loops or bands of which passed off laterally to the sides of the body of the uterus, and formed, as it were, suspensory ligaments. The rectum curved to the left of the tumour, was loosely attached by peritoneum. The tumour was of a fibrous character, interspersed with bony deposits.

The PRESIDENT said, that when the uterus was opened the gush of blood was frightful, but ceased immediately on the removal of the child and the placenta, which were both taken away together. The operation itself was extremely easy, and the patient said gave her but little pain. There was nothing fearful in this operation generally, except the hæmorrhage and the occurrence of after-inflammation. It was interesting to determine when this operation was justifiable, and *when* it should be performed. Might it not have been successful if performed earlier in this case? The prudent surgeon, in cases of tumour obstructing delivery, however, did not feel justified in resorting to the operation early, as the tumour might contain fluid, and by waiting, delivery might be effected by the natural efforts.

Dr. BIRD alluded to two cases in which large ovarian cysts were successfully removed, one by Mr. Waine, and the other by Mr. Clay. In these cases the temperature of the rooms in which the operation was performed was increased to blood-heat. Might not this precautionary measure in some degree influence the success of the operation, and would it not be advisable to follow it in all cases in which the Cæsarian section was performed?

Mr. DENDY agreed in the necessity of avoiding the entrance of cold into all cysts on which we performed operations. He exhibited a drawing illustrative of a case of Cæsarian section which was performed by the late Mr. Bryant. The patient was a young woman, in labour with her first child, in Lambeth Workhouse. There was such an anterior projection of the lower lumbar vertebræ that the space between the sacrum and the pubis was at most but two inches. On a consultation with two or three medical friends (labour having gone on for twenty-four hours) it was determined to perform the Cæsarian section. The first incision, with the view of avoiding cutting the tendon, was made a little to the left of the linea alba. This brought the uterus into view, and an incision, five inches in length, was made into this organ. The child was immediately removed by the feet, the placenta at the same time being detached by an assistant, whilst another assistant attempted to control the hæmorrhage, which, for two minutes, was really frightful; the intestines also protruded, and were returned with difficulty. The hæmorrhage being arrested, the divided parts were drawn together by sutures, the patient was put to bed, and appeared to be doing well. A large dose of opium was administered; the pulse was 110, and good, and did not indicate any serious shock to the nervous system. She slept well that night, and next morning complained of but little pain or uneasiness; she was low, however, and the pulse fluttering. She lived sixty hours, and then died as patients die with puerperal fever. The child was vigorous and healthy, and lived nine days. The examination of the woman was made a few hours after death; there were efforts at union in both the uterus and peritoneum. With regard to the performance of the Cæsarian section in cases in which tumours obstructed delivery, he thought that when these were soft the practitioner should wait for natural efforts; when the obstruction was of bone he should not delay the proceeding.

Dr. BENNETT attributed the success of the performance of the Cæsarian section on the continent to the early time at which it was performed, and its non-success in England to the delay of the operation. The success of operations for the removal of ovarian tumours held out no hope of the Cæsarian section being likewise successful, for in one case the constitution was in a totally different condition than it was in the other. In one everything was made favourable by previous treatment and preparation, in the other everything was against the patient. The occurrence of great hæmorrhage in cases of

the Cæsarian section made the occurrence of inflammation much more probable. In most of the successful cases of this operation there was little blood lost. Sir B. Brodie had, in a late lecture, expressed his opinion in favour of the occurrence of inflammation more frequently after large losses of blood in operations than when there was little hæmorrhage. In Mr. Walne's operation, he (Dr. B.) was struck with the account of the preparation of the room and other circumstances of minutiæ, which, doubtless, had an effect upon the result. In Mr. Walne's and Dr. Clay's cases little difficulty was experienced from protrusion of the intestines, an occasionally very troublesome occurrence. He recollected a case of high operation for stone by Civiale in which the knife was accidentally passed into the peritoneum, the intestines protruded, and were with much difficulty returned. He thought the danger of cutting into the peritoneum had been much overrated.

Dr. WILLSHIRE thought that operations on the uterus were often fatal from the peculiar sedative effect which was produced by making an incision into that organ, an effect which was totally different from that which followed incision into other organs.

Dr. CHOWNE would not resort to the Cæsarian section when there was any *possibility* of bringing away a mutilated child from the mother. The minimum space through which this could be effected was stated to be one inch and a quarter. He did not think the success of the Cæsarian section was attributable to the elevation of the temperature of the operating room, for on the continent, where it was more successful than in England, they did not take such a precaution respecting the admission of cold. He attributed the success of continental operators to the early period at which they performed the operation. There was a great difference between the removal of an ovarian cyst and the performance of the Cæsarian section; the success of the one offered no guarantee for the success of the other. The danger of the Cæsarian section depended much on the effect of the wound of the uterus; the hæmorrhage was usually more alarming than dangerous.

Mr. PILCHER drew attention to the difference of proceeding which obtained in the two cases of Cæsarian section which had been detailed to the society, and inquired if the occurrence of the hæmorrhage and the protrusion of the intestines might not be attributable to this difference?

Monday, Jan. 16.

Mr. PILCHER, President.

INFLUENCE OF EXCESSIVE LOSS OF BLOOD IN THE PRODUCTION OF INFLAMMATION.

A discussion on this point occupied the society this evening. It had its origin in the discussion at the previous meeting, in which a member of the society expressed his opinion that excessive loss of blood was often productive of the worst kinds of inflammation. In support of his opinion he advanced a variety of facts, among others the well-known occurrence of the most fatal kind of puerperal peritonitis in women who had much hæmorrhage at the time of delivery; and the non-success of operations for lithotomy, of those for the removal of ovarian tumours, and of the Cæsarian section, when much blood was lost. Large losses of blood occurring in these cases acted on the system by producing a peculiar kind of irritation very favourable to the occurrence of that kind of inflammation which did not admit of depletion, and which was generally fatal.

It was urged on the contrary side that, *à priori*, it would appear quite opposed to reason to expect that blood-letting, which cured inflammation, could possibly cause it. Was it clearly proved that the diseases mentioned were produced by large losses of blood? It was admitted that excessive losses of blood were injurious, but not to such an extent as was stated on the other side. High authorities differed with respect to the subject, for surgeons of equal eminence and experience were ranged on either side of the question, some taking care to lose as small a quantity of blood as possible in their operations, whilst others allowed of free bleeding as a precautionary measure.

Monday, Jan. 23. The subject was again discussed this evening, but nothing novel was elicited.

WESTMINSTER MEDICAL SOCIETY.

Jan. 21, 1843. Dr. REID, President.

NEW SPECULUM-LAMP.

DR. CHOWNE, in showing a very neat speculum-lamp to the society, made some general remarks on the aid it would afford in examinations, not only of the vagina, but of the organs in which the speculum is used to obtain an augmented extent of vision. The speculum was of very ancient origin, and nothing was known either for what purpose the first instrument was made, or who was its inventor. The principle of the speculum or reflector had not, that he was aware of, been in any instance so fully carried out as in the "speculum ventriculi," devised by Bombobzini for examining the deep cavities, and especially, as its name imported, the cavity of the stomach; to this instrument the term speculum certainly most strictly applied. It was described in the "Dictionnaire de Medicine et de Chirurgie."

A reflecting lamp had also been contrived as an adjunct to the speculum auris, and of those who were in the habit of using a speculum on whatever occasion, there were but few who had not felt the want of a strong light, capable of being both thrown and kept

steady upon the part under examination, or operation, and so arranged, moreover, as not to offer any obstruction to the sight, or impediment to the free and commodious use of the hands in the employment of instruments, &c. The lamp before the society appeared to him (Dr. C.) to possess these necessary qualities. He had used it on several occasions where accurate vision was indispensable, and it had fully answered his purpose. He might add, indeed, that he now considered a speculum examination scarcely complete without it, so much did it contribute to a full and complete investigation. As no description can convey an adequate idea of the lamp, we may remark, that any medical man interested in the subject may see the apparatus at Mr. Miller's lamp-maker, Piccadilly; it possesses, however, the following recommendations:—It is of a very convenient size; it is trimmed with very little trouble; the wick is broad to avoid uncertainty. The light afforded is strong, and by means of a reflector supported on a small, neat, universal joint, might be cast and kept steady upon any particular spot. The reservoir containing the oil is supported in a manner which leaves the sphere of vision perfectly unobstructed, and which also admits of its being suited to various heights. The foot on which it stands is solid, and renders it not liable to be disadjusted by a slight touch while under use. The reflector is of sufficient power to throw the light, even in a strong body, upon an object at a distance sufficiently great to leave abundant room for the employment of instruments. A further advantage is, that while the light is vivid upon the object under examination, the eyes of the operator are entirely protected from it. Dr. Chowne mentioned a curious misapplication of the speculum in forcing open the mouths of those slaves who preferred starvation to slavery, and who were by this instrument compelled to take food.

CIRCULATION IN THE CAPILLARY BLOOD-VESSELS.

Mr. Snow read a paper on this subject. He said there was considerable diversity of opinion amongst physiologists with respect to the forces which circulated the blood. He felt convinced that besides the action of the heart there was one other power of importance engaged in the circulation, a power to which allusion had been made by Dr. Alison and some other authors. He should define it as follows: that the mutual changes which take place at the capillary vessels between the blood and the tissues are attended with attractions and repulsions which assist to impel the blood in a definite direction. According to this view nutrition, secretion, and, indeed, every function would assist the capillary circulation of the part in which it took place. He had nothing to advance respecting the intimate nature of these attractions, and should carefully avoid such terms as chemical, electrical, and vital, both in order that he might not be misunderstood, and because he looked upon chemistry, electricity, and vitality, rather as terms which were useful to us in the infancy of science, than as forces which had a separate and defined existence. The isolated cells of chara, and of most simple vesicular plants, were filled with a liquid which contained globules visible by the microscope. These globules moved round in their respective cells, each cell having a separate circulation. There was no contraction of the walls of the cells, and, indeed, there was no conceivable cause for this circulation except the process of nutrition going on in the cells. In the higher plants, which possessed a special circulation of the sap, there was no contraction of the vessels nor any mechanical force to carry on the circulation. Absorption by the spongioles of the root could raise the sap to a limited extent only, and, indeed, its ascent could be observed in spring to commence at the buds, the fluid lower down afterwards following. Moreover, the course of the sap became changed in the leaves, and the elaborated sap returned towards the roots, often through a great part of its course in opposition to gravity. The various functions of the leaves, and the nutrition and secretion performed with the elaborated sap, were, then, the chief causes of the circulation in plants, this circulation taking place in capillary tubes and spaces. In many of the simpler animals there was a distinct circulation in separate cells, similar to that of the simple cellular plants. Afterwards we came to animals with blood-vessels, in which the circulation was performed solely by the changes which took place at the capillaries; for instance, the trematoda, an order of intestinal worms, possessed two vessels on each side of the body in which the blood moved in opposite directions, and according to the observations of Ehrenberg and Von Nordman these vessels did not contract in the least. Ascending in the scale we came to animals whose circulation was assisted by various contracting vessels; then we had the development of a heart, which became more and more perfect still, in the higher animals, it was an auxiliary to the circulation so important that life could scarcely continue a minute without its action. It was interesting to observe that in some reptiles in which the lymphatic system was very extensive and important, lymphatic hearts became developed to assist the transit of the lymph. The cases of human acephalous monsters without heart proved that two capillary circulations, that of the child and that of the placenta, may be performed by the functions of the parts without any propelling power. In many of these cases there was no other fœtus in the uterus, the

contraction of whose heart might be supposed, by an union of placental vessels, to aid the circulation; and in one remarkable case related by Emmert, the uterus merely contained a well-developed limb, attached to the placenta by a funis. That the action of the heart was of itself insufficient to effect the circulation of the blood was most evident from the phenomena of asphyxia. When those changes in the blood which took place at the lungs were prevented by stopping the access of oxygen, the passage of the blood was at once greatly diminished, and became shortly arrested, whilst the heart continued to act some time longer. Dr. John Reid had found that there was in asphyxia a similar obstruction in the systemic capillaries; the portion of blood which did pass the lungs being but little changed from its venous state could not undergo its usual change in the system at large. The conclusion to be drawn from these facts was, that the change in the blood from venous to arterial, and the opposite change in the body at large, were attended with forces which assisted to pass the blood onwards. Many authors admitted the inability of the heart alone to circulate the blood, but suggested contractions of the capillaries or of the arteries as the auxiliaries. Now, contractions, either of the arteries or the capillaries, would resist the action of the heart as much as they would facilitate the progress of the blood, and consequently could be no assistance unless they were vermicular or progressive; and that contractions of this kind did not exist, the phenomena of the pulse in the arteries and the appearance of the circulation with the microscope clearly proved. The elasticity of the arteries modified the impulse of the heart, and rendered the current of blood continuous; the tonic or vital contractility of the arteries and capillaries likewise had great influence over the state of the circulation, both in health and disease, but none of them could add any force to the circulation. Magendie performed an experiment to prove that the heart was the sole cause of the circulation. He opened a vein in the leg of a dog and noted the flow of blood from it; he then tied the femoral artery, when the stream of blood from the vein diminished, and at length stopped, but after removing the ligature from the artery the blood again began to flow. Now this only proved what no one would attempt to deny, that when the blood was prevented from reaching the capillaries it could not flow through them. These views of the capillary circulation gave a satisfactory explanation of many phenomena in physiology and medicine; for instance, assafœtida, ether, essential oils, and many other volatile medicines, relieved difficult respiration; they were sometimes called antispasmodics, but they gave relief in various cases of obstructed pulmonary circulation, where no spasm was even supposed to exist. Now these medicines were all separated from the blood in the lungs in a gaseous form, along with the excreted carbonic acid and aqueous vapour. By increasing the quantity of gas given off from the capillaries of the lungs they gave an additional impetus to the passage of blood through them, thereby relieving congestion and its attendant distressing symptoms. To call these medicines expectorants would not be correct, since expectoration was not respiration, and it was the latter function they promoted; he should, therefore, suggest that they be called diapnetics, from *dia*, *δια* (through), and *pneo*, *πνεω* (to breathe). He entered at some length into the subject of the function of the skin, and argued that with the hot and dry skin of fever the cutaneous transpiration is not diminished, as is generally taught, but is increased, and that the dryness of the skin is the result of the increased evaporation arising from the exalted temperature. In the next part of the paper he hoped to be able to show that these views gave a better explanation of most of the phenomena of inflammation than did any other opinions.

Dr. Chowne said he rather differed from Mr. Snow in his opinion respecting vitality; he considered that the vital principle was something definite and distinct from every other force, and he could in no respect agree with those who consider that life merely arose from the structure and composition of matter. Mr. Wharton Jones had found that on directing a current of carbonic acid gas on any part the circulation was immediately stopped; he (Dr. C.) could not tell the cause of this curious circumstance. He was unwilling to admit that with the hot skin of fever there was increased cutaneous transpiration.

Mr. H. J. Johnson said he could not comprehend what was meant by these attractions or forces accompanying the changes which take place at the capillaries and assisting the circulation, and he had always felt the same inability to comprehend what Dr. Alison meant. He thought the pathology of asphyxia was but ill understood, and from the facility with which the veins may be injected from the arteries after death he considered the contractions of the heart quite sufficient to effect the circulation. He was of opinion that the arguments drawn from the lowest classes of animals were scarcely valid, since nature often employs means in these animals which she afterwards leaves off when she has adopted more perfect ones; he considered it was so with the circulation, and he believed that in man and the higher animals the heart was the sole cause of the circulation.

Dr. Reid said he could not conceive a cause of motion without some mechanical impulse.

Mr. Snow in reply said, that the circum-

stance of patients drinking a great deal in fever whilst the skin was hot and dry, and getting thinner, whilst all the visible excretions were diminished, showed that there must be increased evaporation from the skin and lungs, but he would endeavour to ascertain the fact by experiment. His inability to explain the nature of the forces for which he argued as assisting the circulation was no proof that these forces did not exist, since we were unable to explain the nature of chemical affinity and various other forces whose existence we admitted. He conceived that all attractions and repulsions, even those exerted at insensible distances, had a tendency to cause motion. He thought that the result of injections after death rather strengthened his views, for those injections had so often been extravasated into the ducts of glands and various places, instead of passing directly into the veins, as to have led authors to imagine various communications which have since been found not to exist. He considered that the analogies from the vegetable kingdom and the simpler classes of animals greatly favoured his argument, and that a close attention to the phenomena in asphyxia, and especially to the circulation in monsters without hearts, amounted to a proof of his views.

ARM-PRESENTATION IN NINE SUCCESSIVE ACCOUCHEMENTS.—This very remarkable circumstance is stated in the "Gazette des Hôpitaux" to have taken place in a woman of large build, usually enjoying good health, and married to a man also of a stout and vigorous frame. In her first labour the presentation was natural, and the child was born without the aid of any medical attendant. But in her nine succeeding labours, all of which took place within the next ten years, the arm almost uniformly presented; and all the children being of large size, only one was born alive. The woman died two days after her eleventh labour.

ROYAL COLLEGE OF SURGEONS IN LONDON.

LIST of gentlemen admitted members on Friday, Jan. 27, 1843:—C. Withington, W. J. Preston, T. W. Benfield, A. Cartwright, S. Byles, J. Prowse, S. Singleton, T. Young, J. R. Lewer, T. Spencer, D. Coulter, C. H. Perry.

BOOKS RECEIVED.

Medical Reflections on the Water Cure. By James Freeman, M.D., Physician to the Cheltenham Hydropathic Institution. London: Saunders and Ottley, 1842. 8vo, pp. 55.

The Annals of Pharmacy, to Feb., 1843. London: J. Churchill.

The Annals of Chemistry, to Jan. 28, 1843. London: Longman.

The Report of the South Shields Committee appointed to investigate the Causes of Accidents in Coal Mines, containing examinations on the subject of Safety-lamps, Ventilation, Instruments, Infant Labour, Education of Officers, Government Inspection, and Medical Treatment after Explosion. With engraved Plates. London: Longman, 1843. Folio, pp. 96.

Dr. McCormack's London and Edinburgh Monthly Journal for Feb., 1843. London: J. Churchill; Edinburgh: Maclachlan.

NEWS AND CORRESPONDENCE.

To the Editor.—Sir: One Dennis Cronin, in June, 1832, obtained a licence from Apothecaries' Hall to practise as an apothecary in any part of England or Wales beyond ten miles of the city of London, and, in the books of the Company, is entered as being resident at Killarney, in Ireland. In 1838 this person appeared in Noble-street, Cheapside, as a surgeon, published a few commonplace observations on the ear, deafness, &c., and professed to have discovered a specific for deafness, which he would make public when he had repaid himself the "expenses" which he said the discovery had cost him; asserting, at the same time, that out of immense numbers only five cases were not immediately relieved, of which he had since cured four, the odd one being in course of improvement. He gave a list of patients, with their addresses, but how few of the patients could be found at the addresses given! Something failed in this scheme, so he visited Cheltenham once a fortnight for some time, and then disappeared. Is this Dennis Cronin the Dennis Cronin of Craven-street, Strand? If so, your readers will know how to appreciate his "cures" of phthisis, and the assertions of his coadjutors, while those who accept his invitations to witness them will do well to bear in mind that another *doctor*, who is fond of inviting the professional world to witness *his* cures in the *eye* line of practice, has been known to employ decoys, to *simulate* the complaint, who were no more afflicted than your obedient servant, AN OLD CONTRIBUTOR.

EXTRA-LICENTIATES.—Sir: Your correspondent (page 604), who reviles extra-licentiates, ought to know that there are country physicians who hold it not as an honourable (yet, in fact, it is now quite as honourable as either an English or Scotch diploma), but as a *legal* document, more valid even than that conferring the fellowship of the college. The licence *intra-urbem* has no legal validity *extra-urbem*; at least it is open to a legal quibble, because it limits the rights of practice to "London, and within seven miles thereof." The licence *extra-urbem* does not limit the rights of the holder, and is moreovergranted in virtue of an Act of Parliament; whereas the validity of the intra-

urbem licence rests only on chartered rights. The extra-urbem licence is, therefore, the only licence the country physician ought to seek. The extra and intra-urbem licentiates of the college are now in a position to each other somewhat like that of the intra and extra-urbem licentiates of Apothecaries' Hall.

A Rural Physician.

Hydropathy. — *To the Editor.* — Sir: I beg to report, through your pages, another instance (which has just presented itself to me), in addition to those I formerly communicated, of the dangers of hydropathic treatment. A female, about 45 years of age, subject to rheumatic gout, was directed, by a practitioner, to have cold water *pumped* daily on her limbs. Not a particle of benefit, as regarded the removal of the disease, or even the most temporary assuagement of pain, resulted; but, on the contrary, a *marked aggravation of the symptoms, with the severest febrile disturbance, ensued.* Under the *ordinary* treatment of rheumatic gout (which it is unnecessary to particularise) the disease is now rapidly leaving her, and Priessnitz and hydropathy have one homicide less to answer for. Your obedient servant,

Robert Dick.

Upper John-street, Golden-square,
Feb. 2. 1843.

Dr. P. Curie has favoured us this week with a defence of the homœopathic treatment,—of the late Lady Denbigh, we presume (not so named, however, in the MS.),—and a rebutment of an assault upon homœopathists which has been printed in a contemporary journal. The communication would occupy six pages of The Lancet, which cannot be spared for its insertion. We have often denounced homœopathy as an engine of quackery, and should do wrong to scientific medicine by taking from it so much space to devote to doctrines that we have thus designated. Besides, homœopathy has, we perceive, a journal of its own. With reference to the paper of Dr. Curie, we can only add that he denies strenuously that homœopathy was blameable for the death of either Lady Denbigh or Madame Malibran, and this may suffice for an answer in our pages to the assertion that the homœopathists were present at the deaths of both patients. If more is required to be said on that point Dr. Curie may print his letter in pages and circulate it, as an advertisement, with The Lancet, on any week that he pleases.

Since the above notice of Dr. Curie's communication was written we have received from a correspondent signing himself *A Homœopathic Physician*, an authentication (which we had required a fortnight since) of a series of statements on the subject of the deceased countess and her fatal disorder, and a third letter, on the same topic, from another quarter, so that the extract of one column into the pages of The Lancet has brought upon us claims for the insertion of at least twenty more. On a re-examination of the subject we find that we can comprise in one column every contradiction that has been given to the original notice; but short as these may be made, they came too late this week for an appearance until the arrival of next Saturday.

A Constant Reader will find the subject of his communication specially discussed, as relates to inquests, in the last number of The Lancet, in which number the attendance upon persons suddenly dead was fully considered. Most certainly the surgeon is entitled to remuneration; but from whom? Why, in law, from the party demanding his attendance. That person is the debtor, if the surgeon obeys the requisition, not the coroner; for the fact that an inquest is held on the body does not transfer the debt from the individual, or the family of the deceased, to the shoulders of the county rate-payers. Surgeons (not under contract) are not legally bound to attend any patients, living or dead. Most certainly, before a medical practitioner does attend he should either consider who will be the debtor for his time and opinion or advice, or be prepared to forego his claim for remuneration, because the summons is hasty, or the parties are poor. But, as regards the coroner, we repeat, that in the present state of the law that functionary would be guilty of a misappropriation of the funds of the county if he summoned medical witnesses with the express object of awarding them a fee for obeying a call to look at a dead body, or for attending a patient just before death, in private practice, whose family are not rich enough to discharge his demand.

Fair Play.—We shall not notice the subject until the law has finally fulfilled its course with regard to the accused person.

We do not understand the object of *A. B's* note, and probably could not accomplish his purpose if we did.

Inquirer.—The plan must be an imposition. All the "extraordinary" methods are absurdities.

Affectus.—The difference is unimportant in every respect.

Lint.—The number will be 12.

We shall be very glad if *Medicus* will hand to us the name and address of any gentleman amongst his acquaintance who is competent to fulfil, and will undertake, at a suitable remuneration, the duty, at least in the quarter whence our correspondent writes.

A Student of Med. (St. Thomas's Hosp.).—Room should be found for exposing the quackery of Mr. Bewley and Dr. Bradshaw, but that similar advertisements flow in upon us so abundantly that, with very few exceptions only, can we notice them.

The other papers referred to by *Mr. Ross* shall receive immediate attention.

Dr. Freeman's letter next week; also those of *Midas* and *X. Y. Z.*